NATURAL SCIENCE:

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NOTES AND COMMENTS.

Natural Science at Oxford.

REMUNERATIVE positions affording facilities for scientific research in Britain are so few, that all who are interested in progress will sympathise with every effort to retain, for purely scientific work, the services of those who have shown an aptitude for original investigation during their University career. There must be Fellowships and Lectureships in the colleges to enable the graduate choosing science as a profession to pursue his work during the interval between the taking of his degree and a possible permanent appointment; and the more of these endowments there are, the greater is the inducement for young University men to aim at something higher than mere honours in an examination. In the interests of Natural Science, we thus desire to call attention to "An Appeal to the Governing Bodies of the Colleges within the University of Oxford," now being circulated by the Linacre Professor of Comparative Anatomy, Dr. Ray Lankester. Though primarily a matter for the University itself, the issues involved have so direct a bearing on the future progress of the biological and geological sciences in this country, that the "Appeal" demands the support of all investigators. The University of Oxford is second to none in the repute of its Professors and Lecturers in Natural Science; its laboratories, museums, and scientific libraries are among the finest; but, if the governing bodies of the colleges continue practically to ignore the School of Natural Science, all these provisions and advantages are in vain. According to Professor Lankester's statement, the graduates in Science *are* ignored to an almost incredible degree, and hence the necessity for a vigorous protest.

It appears that the total number of open Fellowships in the Oxford colleges is at present about 300, and that of these only thirty

are held by graduates devoted to scientific research. Nearly all these, too, are in the hands of physicists and mathematicians; while only one is held by a zoologist. Considering that more than 200 classical and historical scholars are thus favoured, the division of honours is certainly as unfair an arrangement as can well be imagined. We may appropriately ask, with a critic in the *Daily Chronicle* of February 13, whether the amount of research published by Oxford men in classics and history is 200 times as great as that accomplished in zoology; and, with that writer, we may further truly exclaim :—

> "The reverse might not be quite accurate, but it would be most unquestionably nearer the truth. A great mass of Oxford fellows are persons who batten for life on the college funds, because, when boys, they passed a successful examination. Like the Rev. Mr. Pontifex, they obtained honours in examination. They, furthermore, resemble that divine in having afterwards abandoned their studies. At twenty-three years of age it is extremely creditable to have got a 'first-class,' but the reputation so earned can hardly be considered a claim at sixty-three. I have heard it urged in palliation of the inactivity of the classical fellow, as contrasted with the science man, that there is less new work to be done in the former subjects. I beg leave to doubt this; but if it be true, then all the more reason to encourage a subject where there is still a wide field open to the investigator. If the colleges were to bestow their fellowships not by examination, but after the candidates have established some claim by original investigation, there would be a wholesome weeding out of those whose interest in their subject and capabilities were of the purely text-book order. With a test of this kind there would be some hope for the adequate recognition of science, and really able men would be attracted to Oxford, and stay there. Above all, that most delusive weed of university growth, the man of promise, would be rooted out, and his place taken by the man of performance."

As Professor Lankester remarks in his "Appeal," there is not much doubt that financial considerations on the part of those Fellows who are interested in the subjects taught in a college are at the root of the difficulty. Until some change can be effected in these fundamental matters, there is thus but slight hope of a due recognition of Natural Science, which can only be taught in the University laboratories and museums. That the reformation will come sooner or later is inevitable, and we trust its advent may be speedy.

MECHANICAL EVOLUTION.

WE have on previous occasions alluded to the speculations of the American school of zoology in reference to the mechanical origin of the various arrangements observed in the vertebrate skeleton. According to these authors, under the leadership of Professor Cope, there is every reason to believe (i) that living bony tissue is plastic, and therefore readily modified in its form by impacts, strains, friction, &c., and (ii) that acquired characters are inherited. Every process

in the evolution of part of a vertebrate skeleton is thus to be explained on purely mechanical principles.

To the discussion of this subject another interesting contribution has just been made by Professor Cope, who describes two cases of a repaired elbow-joint, one in a man the other in a horse (*Proc. Amer. Phil. Soc.*, vol. xxx., pp. 285–290, pl. ix.). In his well-known work on "Animal Mechanism," Marey has already described instances of the natural healing and repair of diseased or dislocated joints—how a new articulation is often completely formed, not only with accurately fitting faces, but also provided with the requisite ligaments and synovial fluid. Professor Cope's paper thus deals merely with the details of two remarkable examples of this repair, in which a new elbow-joint was completely formed after a dislocation. As the author remarks, if the mechanical necessities of a case of this kind lead to the production of a completely new and finished joint within the space of a few months, "how much more easy has it been for stimuli of allied character to develop the features of normal articulations during the ages of geologic time."

"We have here, also," says Professor Cope, "an instructive lesson as to the matter of inheritance. Everyone knows that mutilations, luxations, &c., are not usually inherited. This is because they are not 'acquired,' in the proper sense of the word. Since characters, truly acquired, are inherited, it is evident that a long continuance of the stimulating cause is necessary to produce a true acquisition. The difference between a character produced by causes apart from the normal life of an animal, and not repeated, and those produced by causes operating daily and hourly for geologic ages, is necessarily very great."

A Glimpse of the Tropics.

In the recent December and January numbers of the *Botanical Gazette*, Mr. D. H. Campbell gives an account of a six weeks' vacation spent last summer in the Hawaiian Islands. In position, these islands are unique, being "more isolated than any other land of equal area upon the globe." They are 2,350 miles from the nearest part of the American mainland, the bay of San Francisco, and about the same distance from the Marquesas and Samoa Islands to the south, and the Aleutian Islands a little west of north. As Wallace remarks in his *Island Life*, they are "wonderfully isolated in mid-ocean," the nearest of the widely-scattered coral reefs and atolls being six or seven hundred miles distant, and all nearly destitute of animal or plant life.

The group consists of seven large inhabited islands and a few rocky islets, extending in a S.E. direction just within the tropic of Cancer. Hawaii, by far the largest and most southerly, is 70 miles across and about the size and shape of Devonshire; the greater part

is, however, rendered sterile and uninhabitable by its active volcano and the lava-deposits. The entire archipelago is volcanic, and separated from the nearest continents by great depths, so the islands must have been always as isolated as at present.

The flora was well worked up by Hillebrand, and, as might be expected, is very peculiar; of 800 indigenous species of seed plants and ferns, 653 or 75 per cent. are endemic; of seed plants alone, 81 per cent.; and of dicotyledons, 85.

The capital, Honolulu, which is situated on Oahu, one of the more northern islands, "is like one great botanical garden." This is said to be largely due to Dr. Hillebrand, who introduced many foreign plants, and his place, kept much as it was when he left the islands, "was a very remarkable collection of useful and ornamental plants from the warm regions of almost the whole globe." Very striking to the traveller from temperate climes is the great variety and number of palms; the beautiful royal palm (*Oreodoxa regia*), with its smooth columnar trunk and its plume-like crown of leaves, betel-nut palms, (*Areca*), the wine palm (*Caryota*), the sugar palm (*Arenga*), and many fan-palms. The young coco palms are beautiful enough, though, unfortunately, very subject to the attacks of an insect which eats the leaves, but in old specimens the trunk is too tall for its girth, so that the trees look top-heavy. The great preponderance of Leguminosæ, especially the sub-orders Cæsalpinieæ and Mimoseæ, is also remarkable. All about the town is the rapidly-growing algaroba (*Prosopis juliflora*) a graceful tree, with fine bipinnate leaves and sweetish yellow pods. The pods are largely used for fodder, and the wood forms the principal fuel-supply for Honolulu. The monkey-pod (*Pithecolobium Samang*), tamarind, and various species of *Bauhinia* and *Cathartocarpus* were also noticed, besides a great number of shrubs and trees, with showy flowers or leaves, mostly familiar from pictures or the greenhouse; such were several species of *Musa*, the traveller's tree (*Ravenala madagascariensis*), and the beautiful *Hibiscus Rosa-Sinensis*. At Puna Hou College is a hedge of night-blooming cereus 500 feet long. Of fruit trees the mango, bread-fruit, and guava are common, also the alligator pear (*Persea gratissima*) and the papaya.

Away from the city the luxuriant vegetation is strange. Along the sea-shore the plain is almost destitute of trees, save for an occasional coco palm, while in the fertile lowlands near the sea are the principal cane- and rice-fields.

The valleys at the back of the city, though very rainy, richly repaid a visit by the luxuriance and variety of their vegetation. Grass-covered hills give way as one proceeds to thickets of *Canna* and a rosy-white *Clerodendron*, while the curious screw-pine is occasionally seen, though much more abundant in some of the other islands. Several showy ipomæas are very common.

With increase of moisture, masses of ferns increase in beauty and number, and at about 1,000 ft. elevation species of *Cibotium*

appear, the genus containing the largest tree-ferns of the islands. Here, too, is seen the tiny *Trichomanes pusillum*, forming dense mats on rocks and tree trunks, and looking like a delicate moss; the full-grown frond is not more than half-an-inch high. Epiphytic ferns also become frequent, species of *Acrostichum*, *Polypodium*, and the beautiful bird's-nest fern (*Asplenium nidus*). Everywhere are thickets of *Freycinetia*, very troublesome to get through.

Of the trees of this lower forest region, by far the most conspicuous is the euphorbiaceous *Aleurites moluccana*, with pale silvery-green foliage; and a little higher up the phyllode-bearing Koa (*Acacia Koa*), the principal timber tree, with wood not unlike mahogany in appearance. Conspicuous also is the mountain apple (*Eugenia malaccensis*), with beautiful crimson fruits, and, higher still, the nearly-related *Metrosideros* forms a striking object with its grey-green leaves and scarlet, feathery flowers.

Hawaii is reached by steamer in about thirty-six hours from Honolulu. On the way are passed Molokai, the barren and forbidding leper settlement, Lauai, and Maui, the next largest island.

Hawaii consists of three great volcanic cones, only one of which is now active. Mahukona, on the leeward side of the island, the first landing-place, showed a forlorn expanse of bare lava, with scarce a trace of vegetation, quite a contrast to the next stopping-place, Hilo, where the luxuriant vegetation comes down to the water's edge.

The forest here is most interesting, and owing to the great annual rainfall (180 inches) and its more southerly position, like the rest of the flora, more tropical in character. Ferns and mosses luxuriate, while flowers are almost entirely wanting. Many tree-ferns, species of *Cibotium*, had trunks from 15 to 20 or even 30 feet high, with fronds 18 to 20 feet long. Growing on their trunks were epiphytic ferns, the peculiar *Ophioglossum pendulinum*, with its long, strap-shaped leaves, and exquisite species of *Hymenophyllum* and *Trichomanes*. Of the terrestrial ferns, the tropical *Gleichenia dichotoma* and *Marattia Douglasi* were noticeable, and several species of *Lycopodium* and *Selaginella* were common.

Coffee is extensively planted in this region as well as on the lee side of the island, and the quality of the berry is exceptionally fine.

Near the volcano, Kilauea, about 4,000 feet above sea-level, are many interesting plants, one a *Vaccinium*, with berries resembling cranberries.

The leeward side of the island is dry and vegetation scanty. The soil, however, is very fertile, and, when water can be had, produces magnificent crops of tropical products, such as pine-apples, coffee, sugar, &c.

A flying trip was made to Kauai, the oldest, geologically, and the richest, botanically, of all the islands. Hillebrand states that not only is the number of species larger, but they are more specialised than in the other islands. Here were seen several woody Lobeliaceæ.

As in all the islands, there is a great difference between windward and leeward sides, the former affording some of the most beautiful scenery and luxuriant vegetation of any seen during the trip.

KIDNEY TUBES IN AMPHIOXUS.

DR. THEODOR BOVERI, of Munich, has made one of the most striking zoological discoveries of recent times. In Spengel's *Zool. Jahrbücher* (vol. v., p. 429), he describes and figures a system of segmental canals in *Amphioxus*. These occur all along the region of the pharynx—one pair for each two pairs of gill-slits. They lead from the body-cavity to the atrial sac. There are 90 pairs altogether: the first and the last open by single ciliated funnels into the coelome; the others by from five to seven funnels. A network of blood-vessels covers the funnels, and Boveri shows that carmine and indigo-carmine taken into the blood reach this network and are removed by the funnels. He gives a series of striking arguments in favour of regarding half the atrial cavity as the homologue of the primitive pronephric duct of Craniata. The whole paper is wonderfully convincing; and one has only to look for a moment at sections of *Amphioxus* to see the canals, and to be astonished that one has not seen them before.

To readers who are not specialists in Zoology, the discovery of kidney tubes in Amphioxus may seem a matter of little moment; but the presence of a definite order and sequence in the development of the excretory organs is one of the strongest links binding together the Chordata—the group of animals which includes the vertebrates and a few lowly forms which have only the gelatinous rod which, in vertebrates, is the embryonic predecessor of the vertebral column. In all the members of this group, the excretory organs first appear as simple tubules, like the tubules of worms. These open at one end by a funnel into the body-cavity; at the other, into a longitudinal duct at each side. In higher vertebrates, like birds and mammals, a complicated series of changes supervenes, and the original excretory organ comes into use as part of the reproductive apparatus while the permanent kidney is developed in connection with the posterior end of the original organ. Probably there is no plainer instance of the embryological law that the individual in its development travels along the path its ancestors traversed in their evolution; for practically every term in the series of changes in the higher vertebrates is preserved as the adult condition in some lower form. It was a puzzle why Amphioxus, which in so many of its organs preserves stages embryonic in higher animals, should be inexplicable in the set of organs most clearly identical in other chordates; and now Boveri has shown that its excretory organs are precisely what analogy and homology alike demand.

More Notes on Seedlings.

The January Bulletin of the Torrey Botanical Club opens with some "Studies upon Akenes and Seedlings of Plants of the Order Compositæ," by W. W. Rowlee, illustrated by five plates. The account, which occupies 17 pages, is much less comprehensive than that given in Lubbock's recent work (vol. ii., pp. 98–161), and the general observations are of less value, being based on fewer experiments. Thus the cotyledons, we are told, vary in shape from spathulate to orbicular, but Lubbock gives several instances of linear cotyledons,—*Bidens humilis*, species of *Ursinia*, and *Coreopsis gigantea*; the American author describes one species of *Coreopsis* (*C. discoidea*), which, however, like another species (*C. laciniata*) included by Lubbock, has broader cotyledons. Nor is any mention made of the interesting polymorphy of achenes, which obtains in the Old World genus *Calendula*. Mr. Rowlee's studies form, however, a valuable addition to our knowledge, as the greater number of his species are not included in the larger work, and the genera are in many cases unrepresented. Moreover, figures are given of every species described, both of the fruit and seedling, and often two or more stages of the latter.

The cotyledons of the Ox-eye Daisy are oblong, like those of the Corn Marigold and another species of *Chrysanthemum* (*C. carinatum*) figured by Lubbock, but Rowlee makes the two following leaves spathulate and entire, thus differing widely from their deeply-toothed character in the other two species. The seedling of the Yarrow corresponds with the description in the English work, except that there is no mention of any hairiness on the first leaves; it would be interesting to know whether they are glabrous in the New World, or the hairs have been merely overlooked. One *Artemisia* is described, viz., Wormwood (*A. Absinthium*), and its seedlings, we are told, may be distinguished from those of the Yarrow by the oblong shape and entire margin of the second and third leaves after the cotyledons. Comparison with the larger treatise shows that this is not a generic distinction, for there we find that in *A. annua* the corresponding leaves are three-toothed as in the Yarrow though in another species (*A. Mutellina*) entire.

The Helianthus (*H. divaricatus*) mentioned bears a general resemblance to *H. cucumerifolius*, included by Lubbock, the cotyledons being ovate and obovate respectively, while in each case the first internode and following leaves are somewhat densely hairy.

In speaking of the embryo, cotyledons and hypocotyl are invariably distinguished, the latter term including everything below the cotyledons, and consequently the radicle. This seems a pity; the radicle is an organ so well marked, not only from a morphological, but also from physiological and anatomical points of view, that it is a pity to ignore or slight its presence in the earliest stages.

Some remarks are also made on the germination of the seeds.

That of annuals and biennials is much prompter than of perennials, while a much larger percentage of the former germinate, giving larger and hardier seedlings.

The percentage of germination was found to be greatest in "persistent field weeds," only two of which, however, were studied; the Burdock and Dandelion; that of wayside and fence-row weeds like Elecampane (*Inula Helenium*) comes next, while in those which cannot be characterised as weeds it is considerably less, the general averages being 49, 33, and 8 respectively. This is taken as direct evidence that the vitality of seeds of a species is a factor in determining its abundance and ability to become a weed. Twenty-five seeds of each species studied were sown with the upper (pappus) end downwards, and a similar number erect; 149 of the former germinated, and 178 of the latter. The radicle emerges from the lower end, and these numbers show that it is a disadvantage for this end to be uppermost, a position, moreover, which the fruit would not naturally take, since the pappus, acting like the feathers on an arrow, tend to keep it erect.

The Hidden Coal-fields in the South of England.

Geologists who are interested in the study of the hidden coal-fields in the South of England will find some exceedingly suggestive remarks in two papers published by M. Marcel Bertrand. The earlier one, though entitled "Sur la Continuité du Phénomène de Plissement dans le Bassin de Paris" (*Bull. Soc. géol. de France*, ser. 3, vol. xx., p. 118), relates largely to the anticlinal and synclinal folds which cross the English Channel and North Sea. We are unable, however, altogether to follow the author when he accepts the present irregularities of the sea-bottom as corresponding to ancient undulations in the strata beneath. It is so rare in our seas to find an undisturbed rocky bottom, except near the Straits of Dover, and shifting sand-banks are so common, that it seems more reasonable to suppose that the irregularities of the sea-bed are mainly due to the scour of the tide. Yet some of these irregularities are undoubtedly ancient hills and valleys, or lines of escarpment.

M. Bertrand's second paper, "Sur le Raccordement des Bassins Houillers du Nord de la France et du Sud de l'Angleterre" (*Ann. des Mines*, Jan., 1893), deals mainly with the hidden coal-fields, their probable position, and their extent. The maps published with these papers are excellent, and ought to throw a great deal of light on the trend of the Coal-measures in our southern counties.

We would also direct attention to Mr. Brady's report on the Dover Coal Boring, reviewed elsewhere.

Prehistoric Archæology.

The "Congrès International d'Archéologie Préhistorique," which met last year at Moscow, has led to the publication of an in-

teresting series of papers on the Quaternary Geology of Russia. The most valuable of these, especially to the foreign reader, will be Professor S. Nikitin's outline of the present state of our knowledge of the subject. In this summary it is pointed out that Palæolithic man existed in Russia contemporaneously with the mammoth during the second half of the Glacial Epoch, but only towards the southern limit of the glaciated area. In the more northern districts all the implements yet found belong to the more advanced Neolithic races. The existence of a distinct Interglacial Period, like that recognised by Scandinavian and German geologists, has not yet been satisfactorily demonstrated in Russia.

It is impossible to criticise this paper without an intimate knowledge of Russian geology, but it may be worth while to remark that in Western Europe also, the relics of Palæolithic man are only found in regions beyond the southern limits of the last glaciation. Is it possible that in Russia, as was perhaps the case in Britain, Palæolithic man was Interglacial? His relics are not found in the areas covered by ice during the last glaciation, and this has been explained as the direct result of the advance of the ice, which ploughed up and destroyed all the pre-existing Palæolithic deposits as far as it could reach.

Our Monthly Selection.

Mr. James Payn has somewhere expressed regret at the supposed fact that men of science are unable to make their subject popularly interesting. It is certainly true that "popular science" does not, as a rule, result from the literary activity of persons qualified to write upon such subjects. We have had occasion more than once to point out that this, if a blessing, is one securely wrapped up; for it is to be presumed that an individual, if he cares to read scientific articles, prefers them to be fairly reliable, else why read an article which is clearly meant to be instructive?

The editor of a recently-started journal, entitled "The Sketch," advertises his willingness to consider paragraphs which are "smartly" written. As an indication of what he wants, attention may be directed to a paragraph in the first number—"Why not a Professor of the Zoo?" The writer talks a little about the parietal eye; for this overworked organ of vision appears to have just filtered down through the "dailies" to the "weeklies." In the course of time it may perhaps reach the monthlies. We present Mr. Payn gratis with the suggestion. The antiquity of the points with which it deals is, however, not the only claim which this paragraph has to be considered smart. Many of us will be astonished to hear that the "hole" in a baby's head at birth is the vestige of a parietal eye! The writer concludes with a lament that there is no professor at the Zoo to make men acquainted with these facts (!). Even though he presumably possesses no parietal eye himself, the two usual organs of vision would

be enough for him to observe that every year for the last—we don't know how many, there have been popular lectures on Zoology given at the Gardens. For the last few years they have been delivered by Mr. Beddard; and it is clear that a course of lectures on Zoology would not come by any means amiss to this paragraphist.

We are always glad to note the gradual percolation of science through the lower strata of our population, and we have derived much instruction from a recently-issued pamphlet entitled "Leaves from the Book of Nature, or Stepping Stones in Creation," by L. Piers. The author seems to be a great admirer of the Geological Department of the Natural History Museum, and his acquaintance with the collection, if not extensive, is at least peculiar. He traces the history of our globe from the "Archaen" rocks "composed of schistose or granitic character," through the Cambrian period when the sea was at 90°, the Silurian and Devonian, with their Graptolites, Ammonites, Crinoids, and Nummulites, the Carboniferous, "Treassic" and Jurassic, then "the Cretaceous system composed of white chalk," down to the Tertiary periods and the great ice age. "Wonderful and marvellous truly are the mysteries of Nature"! thus we read of that "curious crustacean called *trilobite*; they were in three great families, *olenellus*, *paradoxides*, and *olenus*," then the Crinoids, whose "province was to check the too great increase of certain other creatures, while in their turn they were devoured by the larger fishes" [poor fishes!]. But, oh! the "strange and long reptile, named *Deinosaura*," "the *Plesiosaurus*, the earliest crocodile," the "extraordinary *Petrodactylus* and *Trinoceros*."

Two notes on page 16 inform us, first, that "the vertebrate kingdom are in five great divisions: fish, reptiles, birds, mammalia, man;" secondly, that "The visitors will find at the entrance to the Geological Department some excellent illustrated catalogues." Are these catalogues really so bad? or did Mr. Piers write his book before he read them?

It is well-known that a long-haired variety of the tiger ranges at the present day as far north as the region of the Amur, on the frontier of China and Siberia. The animal is thus capable of living in a cold climate, but the astonishing discovery has just been made of evidence of its former range to a northern latitude even within the Arctic Circle. Among the fossil bones collected in the New Siberian Islands and on the adjoining mainland by a Russian expedition despatched from St. Petersburg in 1885, there are five characteristic limb-bones of the tiger. They are described by Dr. J. D. Tscherski in his report on the collection (*Mém. Acad. Imp. Sci. St. Pétersbourg*, vol. xl., no. 1, with 6 plates), and occurred in the same deposits as bones of the musk ox, mammoth, &c.

In the January number of the *Quarterly Journal of Microscopical Science* (vol. xxxiv., p. 317), Mr. Arther Willey has another interesting study on the ancestors of the Chordates. As the result of a series of observations, he groups together *Cephalodiscus* and *Balanoglossus* as Protochordata with mouth ventral, and with no endostyle; Ascidians and Amphioxus, as having an endostyle and a dorsally-situated mouth. In these two groups, *Cephalodiscus* is parallel with the Ascidians. Both are sessile, possess a U-shaped alimentary canal, have a small number of gill-slits (one and three respectively), and reproduce by buds. *Balanoglossus* and *Amphioxus* are also parallel, being free-swimming, with straight alimentary canal, many gill-slits, and without the power of budding. That difficult form, *Appendicularia*, which was long regarded as the most primitive of the Ascidians, Willey considers as a reduced and secondary derivative from the Ascidian stock.

The application of photography to the study of animal locomotion is still being extended. M. Marey, we are glad to record, is continuing his observations on the swimming of fishes, and has just published the result of photographing a skate (*Comptes Rendus*, vol. cxvi., pp. 77–81, Jan. 16, 1893). The body of the fish was firmly fixed, and the motion of the great pectoral fins was photographed from the side and from the front. The undulation of the fins proves to begin at the anterior border, and the whole progress of the motion bears a singular resemblance to the movements of a bird's wing during flight. M. Marey proposes now to devise means of studying the locomotion of the skate under natural circumstances, and will attempt to determine the extent and directions of the currents produced in the water.

There are many strange phenomena connected with the life of protozoa within animal tissues, but a recently-announced discovery by Professor W. A. Haswell (*Proc. Linn. Soc. N.S. Wales*, ser. 2, vol. vii., 1892, pp. 197–199) is, perhaps, the most remarkable of its kind. It is well-known that certain amœboid forms spend the whole of their existence within a single cell of the tissue of their host, and it is the ordinary rule among gregarines to begin life within a cell; but it now appears that even certain freely-swimming flagellate protozoa are capable of passing through life in a similarly restricted sphere. On examining a turbellarian worm from a pond in the neighbourhood of Sydney, Professor Haswell found one or more minute green flagellate protozoa in many of the cells. These were observed to be in an active state, and appeared to be closely related to the familiar *Euglena*.

Dr. Axel Goës has just described, in the *Bull. Mus. Comp. Zool. Harvard Coll.* (vol. xxiii., no. 5), the remarkable new form of arenaceous Foraminifer found during the dredging expedition of the U.S. Fish Commission steamer "Albatross." This form, of which the finest specimen measures 190 mm. in breadth, is by far the largest Foraminifer known. It consists of a "strong network of bundles of very fine chitinous threads, measuring in thickness 0·003–0·006 mm., incorporated with a thin layer of finest sand and *débris* of shells. . . . The test is leaf-formed, with outlines usually describing a triangular, fan-like or reniform figure, with more or less strongly arcuated edge, the whole reminding one of a Padina alga of 0·5 to 2 mm. in thickness." In its earlier stages, this Foraminifer has the fan-like shape of *Pavonina*, but, as the chambers increase in number, they no longer arch over the centre, but terminate with a blunt end at the top, the lower end being produced with long root-like appendages, which "serve probably as fastenings to the botton, where they often are entangled in masses of a Rhizammina." Dr. Goës compares this form with the *Jullienella* of M. Schlumberger (*Mém. Soc. Zool. France*, iii. (1890), p. 211). Dr. R. Hanitsch, on the other hand, writes to *Nature* (Feb. 16) to say that he thinks it is one of the deep-sea Keratosa, and suggests that it may possibly be referred to *Stannophyllum zonarium*, Haeckel.

We understand that advices have been received from the Villiers Expedition to Lake Rudolph, dated Vitu, December 20, 1892. Lieut. Villiers has had fever, and the command has fallen on Lieut. Stanford. Mr. J. W. Gregory is reported to have been in excellent health and spirits, and the expedition was to be on the march in a few days. Subsequent statements, however, have appeared, which show that Lieut. Villiers has recovered, and has joined the Portal expedition to Uganda.

We are glad to observe that, since our last issue, Professor Lapworth has closed the controversy on the geology of the Scottish Highlands, by contributing to the *Daily Chronicle* of February 8 a concise historical statement which we can endorse.

I.

The Nucleus in some Unicellular Organisms.

THE importance of continuous and exhaustive studies concerning the ultimate and irreducible nature, morphology, and function of animal and vegetable cells is universally seen. The cellular elements of tissues began to be observed even by Hooke two centuries and a quarter ago; and some further advances were made, but it was not until 1831 that Robert Brown, the great pioneer in botany, took the first great step leading to a practical advancement of the subject. He gave definite knowledge of vegetable cells, and he demonstrated that the nucleus was one of its normal elements.

It need hardly be remarked that the cell-theory proper had its foundation in the work of Schleiden, but by him it was not extended beyond the structure of plants; he clearly defined the vegetable cell as the elementary organ which constitutes the sole essential form-element of all plants, and without which a plant cannot exist; and as consisting, when fully developed, "of a cell-wall composed of cellulose, lined with a semi-fluid nitrogenous coating."

The cell was thus to Schleiden a vesicle with semi-fluid contents.

In the year following (1839) Schwann showed that the Animal Kingdom was ultimately as cellular in structure as the Vegetable; but to the vesicular wall and the semi-fluid lining of Schleiden he added a third element, the *nucleus*, and he deemed this essential to the history of the cell, at least in some period of its life.

From this time, the triple elements of the cell were accepted as its normal condition; but investigation made the continuance of this belief more and more uncertain. It was shown that cells multiplied by "budding," and that the nucleus underwent fission when the cell divided; and also that no cell could take origin save from a parent cell.

Soon it was demonstrated that there was no vital importance attachable to the cell-wall; and in 1857 Leydig declared it entirely unessential, and defined the cell as a "soft substance enclosing a nucleus."

Subsequently, Max Schultze contended that the life of a cell might be complete without a nucleus, but the cell was held to be the ultimate morphological unit in which life was manifested. Every

living thing presented itself as a sum of vital unities, any one of which manifested the properties of life.

The living matter of the cell was held to be *sui generis*. Not-living matter could not, by any combination we knew of, apart from the genetic intervention of living things, take on its unique properties. Only from the living could the living arise. This living matter was, and is, called protoplasm; and even now a cell can hardly be more clearly defined than as a small mass of protoplasm enclosing a peculiarly organised element known as the nucleus.

The further study of cells, aided by the constantly increasing power and precision of our microscopes, revealed complexity, reticulation, striation, foliation, radiation, and other appearances, at present undetermined by the best observers with the finest accessible optical powers; while the nucleus is seen to be highly complex, and subject to the most striking internal modifications during its cyclic changes.

To pursue or even indicate the history of the prolonged and untiring work upon the cell during recent years is no part of the object of this paper; enough to recall that the nucleus is found to be the initiating centre of all the great changes which the cell undergoes.

But the study of the cell has been conducted in the main (1) upon cells belonging to highly complex organisms, and, therefore, as vital unities of that organism must partake, in all their mutations, of the complexities involved in the developmental organism of which they are units; and (2) the different processes of cellular change and nuclear modification have been studied, of necessity, after death, and under the influence of desiccation and staining.

For years it has impressed itself upon the mind that the great problem of the nature and behaviour of the cell should at least be begun where the problems to be solved ought to present themselves in their simplest condition, and that would appear to be amongst the unicellular organisms—the organisms whose complete life, when traced through all its cyclic changes, begins in a cell and ends in a cell, presenting an elementary though, in their case, a permanent condition in the evolutional history of more complex organic forms.

These are easily and everywhere accessible in that remarkable but minute group of organisms known as *Saprophytes*. They succeed the saprophytic Bacteria in the destructive fermentation of large masses of dead organic tissue. There are only a few of these forms known, and their successive action upon the decomposing mass is both mechanical and physiological.

They are strictly unicellular; the majority are nucleated cells; but the problem of the cell is rendered the more interesting by the study of them, from the fact that amongst the group are distinctly non-nucleated forms.[1]

[1] *Vide* the author's researches, *Month. Micro. Journ.*, vols. x., xi., xii., xiii., xiv., xvi.; also *Proc. Roy. Soc.*, 1878, and *Journ. Roy. Micro. Soc.*, ser. ii., vols. v. and vi.

As typically represented, they are oval or suboval, are possessed of one or more flagella, and have an average long diameter of about the 1-4000th of an inch.

Two fair representatives of the group as re-examined during the last four years with the advantage of our immensely-improved object-glasses and eye-pieces are represented in Fig. 1. A represents a form about the 1-4000th of an inch in long diameter, and B a kindred form the 1-5000th of an inch measured in the same way. They are in the drawing magnified 1,800 diams.

A is distinctly nucleated, and trails a flagellum when swimming, but (as in the drawing) often "anchors" it to the "floor" of the stage; and from this position acts by springs upon the masses of decomposing matter around it. The form B simply swims either forward or backward by means of its flagellum at either end.

The life-cycle of A may be taken as representative, and will show what was actually known by us before the investigations upon the nucleus made within the last four years with our present apochromatic microscopic illumination and apochromatic objectives.

Taking A, Fig. 1, as the normal form, in certain conditions of the organism now definitely known to us, a sudden opening of the "beak" to which the flagellum is attached (*a*, Fig. 2, C) would arise partly dividing the flagellum, as shown in the figure. While this was increasing, so far as we could discover in our earlier studies—in the course of a couple of minutes—the nucleus C showed a definite dividing line, and the "trailing flagellum" also rapidly split, as at *b*.

The division first initiated widened (D, Fig. 2, *e*), and the front flagellum became wholly divided, the trailing one not splitting so fast; but the nucleus showed, as at *d*, a strong tendency to fission.

Very rapidly the bisection ensued, until generally, in not more than four minutes, the division had been wholly effected lengthwise, as in Fig. 3, E, *g*, *h*, and the nucleus was wholly divided save for a fine thread connecting the parts, as at *i*, K, and the trailing flagellum was nearly divided.

Soon the protoplasmic neck connecting the dividing forms was almost wholly gone, as in Fig. 4, F, and the two nuclei, *n*, *o*, were as perfect as in Fig. 1, A, that is to say, a highly refractive body with a distinct envelope, or border; and the two perfect forms, *l*, *m*, Fig. 4, become disconnected, dividing the trailing flagellum to its root, and each being free.

This process is exhaustive in the great majority of the forms. By following successive fissions—that is, one-half of each division—it was found that the end came at from four to five hours. In other words, this process of division ceased by the death of the last-divided form. But this only applied to about eighty-five in every hundred. The remaining fifteen go through a very curious change.

Fig. 5 represents a final form derived by fission. Its chief distinction is a strong nucleus, and always a free-swimming condition.

In the course of from five to seven minutes there would supervene an amoeboid condition, not striking, but still visible, as shown in H, Fig. 6, where the body-substance would oscillate from the hollow condition shown to the lateral expansions indicated by the dotted lines at *s*, *t*, a vacuole—curiously like an "eye-spot"—would appear, exhibiting very marked opening and shutting, as at *v*; and the nucleus, *u*, was driven to the middle of the body, greatly enlarged, and, losing its nucleus-like aspect, was torn apart in irregular halves; meanwhile, a very strong sigmoid division-line, shown in J, Fig. 7, at *w*, *x*, became extremely marked, and the divisions of the nucleus were pushed one on each side of this line, as at *w* and *x*. At this time a point of protoplasmic material was pushed out, as at *y*, dividing into two fine threads.

A gliding motion now ensued, by which *w* glided off from *x*; this is seen at Fig. 8, κ, α, β, showing two normal forms nearly freed from each other in this way; but in our earliest work it was quite manifest that the nuclei of these two forms had quite strikingly changed, having undergone some radical alteration, and trailing flagella were formed at *z*, partly by growth and partly by division.

Soon after total freedom from each other, these modified forms swam freely for some minutes, and then directed their motion to some part of the field where the ordinary forms (with the usual nuclei) were in greatest abundance, and almost immediately what was seen to be an act of fusion commenced. The initial stage is shown in Fig. 9, L, where the unlikeness of the nuclei is seen at γ, δ.

The fusion takes place rapidly up to a certain point, for in eight minutes a large part of the body-substance of each has fused with the other, as seen in 10, M, where the nuclei are fully united, and a radial or star-like diffusion takes place, as seen in η; and this continues, growing fainter and less perceptible for an hour, or sometimes an hour-and-a-half, until it wholly disappears, and all trace of nucleus is gone, the mass of the plasm of the combined cells being faintly granular, or perhaps reticulated; but the trailing flagella (θ) fuse together and unite with the body, which now rapidly changes, so that in the course of fifteen minutes it assumes the shape and condition shown in Fig. 11, N. At the end of four hours a kind of pulsating movement shows itself in this triangular sac, and at length it burst one or two, and sometimes (as in Fig. 12, *o*) the three corners of the triangular sac, and an enormous number of semi-opaque and extremely minute particles, escaped (Fig. 12), and by watching these they were seen to pass by growth into forms corresponding to the common form from which they arose.

These observations were made in the original and renewed study of the behaviour of this organism as a whole; but during the past three years the greatly superior microscopic apparatus, both illuminating and image-forming, placed at our disposal, has led to an

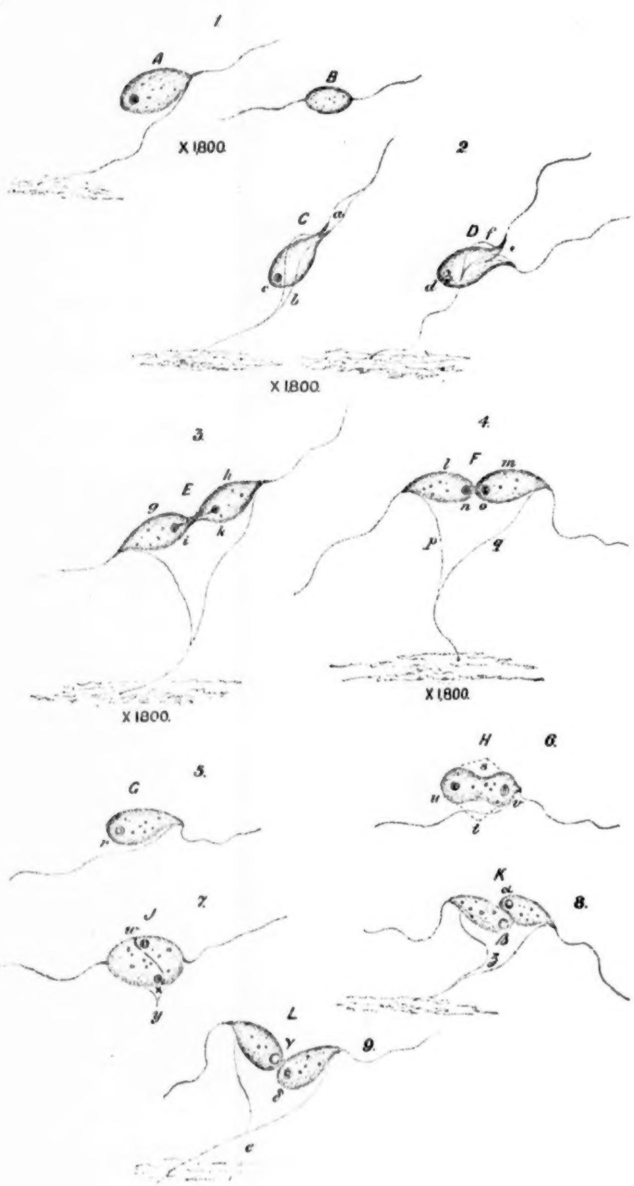

PLATE I.

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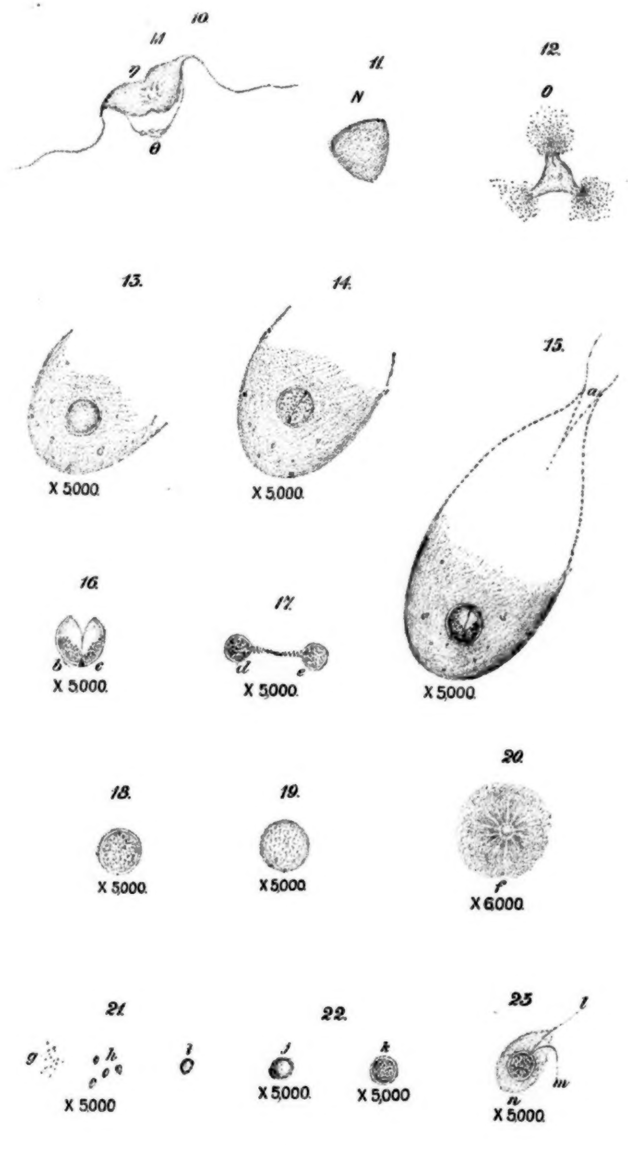

PLATE II.

To face p. 176.]

DESCRIPTION OF PLATES.

PLATE I.

Fig. 1.—*A. Heteromita rostrata* (Kent).

„ 1.—*B. Monas Dallingeri* (Kent).

„ 2.—*C, D. H. rostrata* undergoing fission. This was seen in earlier studies to begin in the body, and apparently afterwards (as in *c, d*) to extend to the nucleus.

„ 3.—Almost complete division of the body and the nucleus of the same; but a fibrous connection exists between the two parts of the nucleus.

„ 4.—Total severance of the nucleus, and the division of the body and the flagella is almost complete.

„ 5.—*A H. rostrata* produced by fission.

„ 6.—The final product of a long series of fissipartitions undergoing the change preceding sexual fusion.

Figs. 7, 8.—The process of fission in this special form.

Fig. 9.—The initiation of fusion in an ordinary form with one of the above.

PLATE II.

Fig. 10.—Early result of fusion of two of these forms; the nucleus has blended, and exhibits a radial condition.

„ 11.—A sac resulting from the complete genetic fusion of the two individuals.

„ 12.—The ripe sac bursting and emitting minute semi-opaque spores.

„ 13.—A study with great magnification of the nucleus separately. This represents the nucleus before any evidence of change is discoverable.

„ 14.—The first evidence—seen only in the nucleus—of approaching fission in the organism; beaded threads appear throughout the plasm; and a line shows itself incident with the long diameter of the organism.

„ 15.—The fission begins in the body at *a*, but has been preceded by a condensation of the beaded structure to the right and left of the division-line.

Figs. 16, 17, 18.—The nucleus seen in process of fission, almost divided (as seen in Fig. 3, pl. i.) and immediately after division.

Fig. 19.—Nucleus after division in the exceptional form shown in Fig 8, *k*, pl. i.

„ 20.—Condition of the nuclei in the fused forms.

„ 21.—*g, h, i.* Growing spores.

„ 22.—*j.* The spore grown to the size of a nucleus, a pause takes place in actual enlargement by growth, and internal development of the beaded structure arises as at *k*.

„ 23.—After the internal development of the nucleus the body begins to great apparently from the nucleus, and the flagella, *l, m*, are pushed out; after this, growth to the normal size is rapid.

endeavour to study alone the behaviour of the nucleus, both before and during, as well as after, the various steps in the cyclic changes undergone by this cell.

A magnification of from 5,000 to 6,000 diameters was found efficient for this, and with the life-history of the cell known, it was less difficult to trace by themselves the nuclear changes.

In studying these, it is only the changes occurring and observed in the living organism, and during its life, that have proved of any real value. The effect of desiccating and staining these saprophytes was in all senses unsatisfactory; the details were too minute, or too delicate to admit of this treatment, for whatever method was adopted, the results, as compared with the living form in the same stage, were simply unintelligible and useless. But this could only be really seen by *comparison* of the results of the same phenomenon in the living organism, and in the dried and stained state.

Nevertheless, by using a one-and-a-half per cent. of acetic acid, to which varying quantities of methyl-green are added, a decided strengthening of the special nuclear features was effected. Thus the nuclear envelope seen in most stages of its activity is much strengthened, and so were the beaded threads embedded in the nuclear hyaloplasm; and little, if any, inconvenience could be seen to result in the organisms.

In the inactive stages of the organism, or when no cyclic changes were manifest, the nucleus was hyaline, or at least exhibited nothing but the faintest traces of a colourless reticulation when very critically examined. This simple state of nuclear hyaloplasm is seen in Fig. 13, representing a portion of the body of the organism and the nucleus, the latter magnified 5,000 diameters.

By steadily watching a form in this condition, it would in all probability be seen to become more marked in its granulation, and at length what appeared like a delicate thread of beaded structure became more and more visible, occupying at first the circumference and at length the whole plasm of the nucleus, as shown in Fig. 14; but when this was complete a distinct line, made more visible by methyl-green, appeared, as shown in the nucleus of that figure. No indication of any division anywhere in the body of the organism had yet appeared. Now, however, it soon becomes manifest that the complex foldings and twistings of the beaded thread in the nucleus condense on either side of it, leaving the dividing line in a hyaline space, as shown in Fig. 15; and now for the first time a line appears at *a* and opens, so that the fission of the *body* has begun.

After this the process is rapid, occupying only three or four minutes. The nucleus opens as in Fig. 16, where it is seen apart from the body, corresponding to the stage seen in Fig. 2, D, rapidly passing into the condition seen in Fig. 17, corresponding to the state shown in Fig. 3, E, and where the fine-beaded thread appears to be drawn out, connecting the still dividing nuclei; but in this

condition the convolutions of the beaded thread have again distributed themselves over each divided nucleus, and soon enlargement and total separation take place, as in Fig. 18.

In the divided organism containing this organism the division-line soon again appears in the nucleus, and successive fissions go on. But in the nuclei belonging to the condition of special fissipartition shown in Fig. 7, the nuclei present quite a distinct appearance. No wall is at all discernible in connection with it, and it is slightly larger than the ordinary form; it is highly refractive, and, on critical examination, does not show the exquisitely fine convoluted beaded thread of the usual nucleus, but dissociated white, and slightly darker, somewhat oblong granulations, as shown in Fig. 19. When in the usual order, in the act of fusion shown in Fig. 9, L, this nucleus comes into contact with the ordinary one (γ, δ), there is almost immediate "melting of either into other," the whole mass resulting being milky and almost opaque, taking a white star-like appearance, resulting apparently from the diffusion of the whole mass through the entire plasm of the fused organisms, for in the course of a short time it disappears, and, as in Fig. 11, N, a non-nuclear triangular body results.

The bodies ultimately emitted from this are of extreme minuteness, and partially opaque; and in their growth one of the most remarkable features appear.

In Fig. 21, if *g* represent the original spores as sent out into the fluid, *p* will represent their growth in form and relative size in an hour, *i* shows one of the same in an hour-and-a-half, while *j* (Fig. 22) gives the result of growth at the end of nearly three hours; but now very little advance in size is made; there is a pause, as if the form had been arrested in growth by death. But, on critical examination, there is found to be a very slight enlargement taking place, and with it the formation of the *internal* beaded thread-like convolution which may take thirty to fifty minutes to complete, as in Fig. 22, K.

From this time growth is rapid; but it appears to proceed from within the nucleus outward, as shown in Fig. 23, where a small part of the body-substance has arisen outside the nucleus, *n*, and, at this stage, the two flagella are seen as though emerging from the nucleus, as at *l*, *m*. The body-substance now rapidly grows until the adult form and size are reached, as in Fig 1, A, when the processes of the life-history once more repeat themselves.

It would thus appear that every point in the cyclic changes in this organism originate in, or are initiated by, the nucleus. Fission is not only indicated, but begins first in the nucleus, while the nuclear changes during fission are worthy of careful note. The change in the character of the nucleus in certain forms after fission is full of suggestion, and that their subsequent union with nuclei of the common type is equivalent to fertilisation, we can scarcely doubt. Not less interesting is the origin of the nucleus in the growing form, and its relation to the growth of the body.

The non-nucleated saprophytes (as Fig. 1, B) may, perhaps, in some sense, be looked upon as wholly nuclear, although I have been unable to discover anything but the most faint traces of structure within them. They present the constant appearance of the nucleus when in a totally inactive state, as shown in Fig. 13.

This appears to present the activities of the nucleus in its simplest condition as accessible to our present means. That there are phenomena far more delicate than those here recorded, we can scarcely doubt, but it would appear that it is in the unicellular organisms that the first lessons in cell-history may be learnt.

W. H. DALLINGER.

II.

Are Great Ocean Depths Permanent?

IT has been suggested by the editor that English readers would like to hear my views on that much-debated question, the Permanence of Ocean Basins. I am rather at a loss how to deal with the subject, because this question involves so many difficult chapters in the history of our planet, and because I regret to see that discrepancy of views exists on fundamental principles.

Mr. Wallace begins by arguing from the principle that "on any large scale, elevation and subsidence must nearly balance each other, and thus, in order that any area of continental magnitude should rise from the ocean floor, ... some corresponding area must sink to a like amount." I venture with deference to reply that I cannot agree to this. It seems, on the contrary, as if two different types of movement had been going on since the first formation of the terrestrial crust. In the first place, there is folding, recently explained in a masterly way by Professor Lapworth, in his Address to the British Association. Secondly, there is the sagging-down or "effondrement" of smaller or greater parts of the crust, caused by the progressive diminution of the planet's radius. *This descent of parts of the earth's crust seems to be the true origin of the great oceanic basins.*

Sometimes the contour of the sunken area follows the trend of a folded mountain chain; at another time it may cut right across it. In smaller examples the outline very often takes a more or less irregularly circular or elliptical form. The descent of a considerable area, forming a large new depression, demands a certain part of the existing volume of oceanic waters for the filling of the new depth. The consequence is the sinking of the oceanic surface all over the planet, and the *apparent step-like rising of coast lines.* Thus is explained the apparently episodic elevation of whole continents, without any disturbance of horizontality, or the least alteration of the net of watercourses spread over the land. It is in this sense alone that a certain balance of "elevation" and "subsidence" might be conceded.

In the entire Pacific region the limits of the oceanic basin are traced out by the trend of long mountain folds. So it is from New Zealand and New Caledonia to the borders of Eastern Asia, to the Aleutians, and all along the western coast of both Americas. This is not the case in the Atlantic, nor in the Indian Ocean; here the

coasts are not outlined by folded structure, except in the arch of the Lesser Antilles, and in the corresponding short arch passing through Gibraltar, which serves to connect the mountains of north-western Africa with those of the south of Spain. These are what we may call the Pacific and the Atlantic types of oceanic regions.

Indian geologists have shown how the immense Asiatic mountain waves, moving southwards against the Peninsula, have been dammed back by the resistent Peninsular mass, the Koráana Hills, and the wedge-shaped mass of Assam; so that they actually form distinct arches, separated by deep angles receding to the neighbourhood of Tank, north of Dera Ismail Khan, to the Upper Jhelum and Brahmaputra Valley. In this case we call the Peninsula the "Vorland."

Now cast a look on the map of the North Pacific, and compare the receding angles which mark the western and the eastern ends of the Aleutian arch, where it abuts against Kamschatka and North-West America. You will remark that this part of the Pacific is a "vorland," and homologous to the Indian Peninsula, whilst the Yellow Sea, Behring Sea, and others lie within the folded region. You may also examine the Mediterranean, and observe that the western half lies wholly within a curved and folded mountain chain (Apennines, Sicily, North Atlas, Gibraltar, Andalusian Cordillera), and that in the eastern half all the part south of Crete and Cyprus and east of Sicily lies on the African "vorland," and the rest on the sunken parts of the Tauro-Dinaric arch.

In the Atlantic region the mountain folds, as a rule, break off against the ocean (*e.g.*, Brittany coast, Devon and Cornwall, south-west Ireland), or else have their folds facing away from the ocean, as in the case of the Alleghanies, and all other folded zones on the eastern side of North America as far as Newfoundland. The folds disappearing in south-west Ireland and in Brittany so very much resemble those rising from beneath the Atlantic on the coasts of Nova Scotia and New Brunswick, that M. Marcel Bertrand has ventured to publish a sketch-map, showing these chains trending right across the Atlantic.

There exists a curious tendency for a depression or a sort of valley to form in front of the great folds facing the "vorland." For instance, the depressions of the African desert in front of eastern Atlas, the river valleys in front of the high Indian chains, and the Persian Gulf in front of the Zagros chains. Quite recently the Austrian exploring ship "Pola" found a depth of 4,400 metres near the south-west coast of Greece, near the front of the Dinaric arch; and some of the greatest oceanic depths show exactly the same position in front of the arches of Japan, the Kuriles, and the Aleutians with Alaska. This is the homology, for example, between the Ganges valley and the Tuscarora depth, both marking the limit of the folded ranges and the "vorland."

The structure of the earth's crust does not, therefore, tell us that

the great depths must be very old or permanent; so let us compare the character of the sediments near the existing coasts.

The Old Red Sandstone is an extra-marine formation; yet the Old Red Sandstone runs out into the sea in the Orkneys, reappears in the Shetlands, and the same palæontological and extra-marine character is known in Spitzbergen. Old Red exists on the north coast of Lapland, and on the White Sea. The plant-bearing beds of the Karoo run out to sea in British Kaffraria; they are repeated in India and Australia. The fresh-water beds of the Wealden pass over from England to the Continent; they not only reappear in Hanover, they run out into the Atlantic in the lower Charente, and on the coasts of Spain and Portugal. Why must the continent which formerly bounded all these vast fresh-water basins have been limited within the existing 1,000- or 1,500- or 2,000- fathom line? The breaking down of the bed of the Ægean Sea, described by Spratt and Neumayr, is of Post-pliocene date, for Pliocene fresh-water deposits form parts of the coast; and yet the depths go far beyond 1,000 fathoms. In 1892 the "Pola" measured 3,591 metres in lat. 35° 52 36", long. 29° 1' 24" E., quite near the south-west coast of Asia Minor, and close to the mighty Ak Dagh (10,000 feet); and this although the separation of the neighbouring island of Rhodes is so recent, that not only do the Pliocene fresh-water beds pass over from the Continent, but according to Bukowski also considerable masses of Middle Pliocene fluviatile conglomerates, originating in Asia Minor, have been deposited by a great river on this island.

Now suppose the existing quantity of oceanic water to decrease, say by evaporation into the ether, as Zöllner once thought, or in any other way; we might by this gradual diminution of the entire quantity attain a beach-line 500 fathoms below the present shores. The continents would appear so much higher, and dry land would extend. Plains would successively appear, more or less similar to Holland, and our present rivers Shannon, Seine, Loire, &c., would flow through these plains. In most cases the rivers would be caused to cut back their valleys, new transverse and parallel lines of erosion would appear, and the plain would be diversified into hills and valleys. The hills south of the Shannon would probably show the rest of those anticlines and synclines which dip below the ocean in south-west Ireland, and we should be able to see a greater part of the northern Armorican arch. The Scilly Islands would appear as another granitic laccolite within the continued Armorican region of Cornwall. The gneiss of Eddystone would come up within this northern Armorican arch, exactly as the gneisses of the Alps stand up within and behind the folded arches. In a similar way, in the south, the anticlines and synclines of French Armorica, which disappear north and south of Brest, would begin to be visible; but in the north-west of Ireland we should see a plateau, ending in a steep cliff, the abrupt boundary of a deep channel, separating the great island of Rockall

from the European continent. And all this varied new-born land would be green and full of life, and people would not be at all willing to believe that it ever was not so.

Then we would descend to the new shore, and one of our great masters would tell us that ocean depths are permanent, that is to say, from 1,500 fathoms downward, or from 2,000 fathoms. The general impression resulting from the study would be the same as now, but the assumed permanent level would be reduced by 500 fathoms.

We might invite our master to undertake an excursion with us; we would go to Scotland. An isthmus, some 1,200 feet high at the narrowest part—Sir Wyville Thomson's ridge—would lead us first to some isolated peaks, nearly 3,000 feet high, and over a rising country to the high peaks of the Faröes. We would observe land-born coal-beds between the great coulées of basalt. Proceeding further, we would travel to the north-west, over a broad tract of rocky land some 1,200 or 1,300 feet high; then first meet an isolated mountain of about 1,800 feet, and from there ascend to the volcanic mass of Iceland, where we should see those vast fields of lava, dotted with active volcanoes, and observe the long faults and open rents cutting through the masses of lava and trending across Iceland in a broad arch, first from south-west to north-east, then northward, beyond the volcano Askja to Husavik; and beyond this broken and breaking zone we might gain the great "effondrement" or "Kesselbruch" of Faxafjord, beset all round by volcanoes and hot springs, from Snaefells Jokull to Reykjavik. In following the rents so well described by our indefatigable Thoroddsen, we might detect faulted plant-bearing beds, and recognise the equivalents of the Faröe coal-seams. If some younger and more impressible student be in our company he might well exclaim, in face of these plant-bearing beds, stretching on to Greenland and showing the existence of a vast dry land in late times, and in face of these rents and volcanoes: "Verily, Professor, the sagging-down of the North Atlantic is the most recent event; it is going on before our eyes; and as the highest mountain chains are the youngest, so also are the deepest parts of the planet the most recent." I fear I should not know how else to answer the student than, "Really, I do not know."

Now let us quit the coasts and examine the interior of a great continent.

Modern geology permits us to follow the first outlines of the history of a great ocean which once stretched across part of Eurasia. The folded and crumpled deposits of this ocean stand forth to heaven in Thibet, Himalaya, and the Alps. This ocean we designate by the name "Tethys," after the sister and consort of Oceanus. The latest successor of the Tethyan Sea is the present Mediterranean.

I asked Dr. Diener, recently returned from India, to give me his estimate of the thickness of the deposits in the Silakank region. Dr. Diener answers: From Dhauli-Ganga Valley, between Gweldung

and Pethathali encamping ground above Silakank (19,265 English feet), to Sirkia River in Hundes; a complete section from (Cambrian?) Haimantas to the Gieumal Sandstone (Cretaceous), without a great discordance, gives, according to Dr. Griesbach:—

Haimantas	3,000—4,000 feet.
Lower Silurian	200 ,,
Upper Silurian	1,100 ,,
Devonian	700 ,,
Carboniferous	1,200—1,400 ,,
Permian and Trias	3,600—3,900 ,,
Lias and Spiti Shales	1,400? ,,
Gieumal Sandstone	1,200—1,500 ,,
	12,400—14,200 feet.

The determination of the thickness of the Spiti Shales and Gieumal Sandstone is difficult, because these less-resistent beds are crumpled into local folds.

A parallel section across the Kurkutidhár range (Chor Hoti, about 18,000 feet), Shalshal encamping ground and Shalshal Pass (16,390 feet) to Hundes gives, without the Haimantas:—

Silurian	1,200—1,400 feet.
Devonian and Carboniferous	1,800—2,300 ,,
Permian and Lower Trias	200 ,,
Middle Trias	100 ,,
Upper Trias to Dachstein Limestone	1,400 ,,
Dachstein Limestone, Rhætic and Lias	2,200 ,,
Spiti Shales	1,000? ,,
Gieumal Sandstone	1,500? ,,
	9,400—10,100 feet.

These figures show that a great and deep ocean has been incorporated into the continent, and that the deposits of this ocean form part of the highest mountain ranges.

It may be remarked that within the eastern Alps Mesozoic limestones of different ages contain deep-water radiolarian chert. But the great and well-bedded masses of white Rhætic limestones of Austria betray distinct proofs of a continuous rising of the shore-line. It is also true that certain bright red enclosures within these white limestones seem clearly not to be red deep-sea clay, but true *terra rossa*, formed by atmospheric decomposition of the limestone; so that these beds must have formed reefs in the ocean. Therefore it is at present difficult to say whether in the Alps the Tethyan Ocean did at any time attain the total depth of, say, 2,000 fathoms; or whether deposits followed so rapidly and depression was so continuous that this was not the case.

The later Tethyan history, the recapitulation of the vicissitudes which led to the formation of the existing Mediterranean, forms certainly one of the most attractive chapters of historical geography. Marine deposits of Mediterranean type (Erste Mediterranstufe, Miocène inférieur) enter the Rhone Valley, surround the present site

of the Alps, and continue far away to the East, to Persia, and have been met with by Griesbach near Balk, on the Oxus. Then these deposits were folded, in the Taurus, in Asia Minor, in Switzerland. Afterwards came the sagging-down of certain parts of these folds, near Vienna, in Hungary, &c., and all that varied series of consequent events. After the first Mediterranean came the formation of an immense horizon of salt deposits, stretching from Wieliczka to Persia; then a second Mediterranean reaching far into the newly-formed depressions; then the appearance of vast fresh-water lakes, lasting through a long period of time till the breaking down of the Ægean land and the re-conquest of the Black Sea.

Look at smaller examples of such partial subsidences; see Margerie's instructive paper on the Corbières, showing the sinking down of the Pyrenees, Miocene beds passing beyond Narbonne, while south of Cape Leucate two more recent Pliocene "effondrements" form the Rousillon, described by Depéret, and the Golfe de Rosas.

But this is only part of the Tethyan history. Michelin's and Duncan's palæontological studies in the West Indies have revealed the European character of certain deposits. It is the "Gosau type" of the Cretaceous which appears in Jamaica, and the Castel-Gomberto horizon of Upper Oligocene (warm type of Sables de Fontainebleau) is known in several other isles. In regions still further off, one of our first masters, the venerable Dr. Philippi, has shown that the present molluscan fauna of the Chilian coasts is of quite recent origin, and that until the beginning of Quaternary times the European Mediterranean molluscan types stretched far down the western coast of South America. At the same time the Mesozoic deposits of Chili, and those recently discovered at Taylorville in California,[1] show purely European characters, and the Neocomian of Bogotá is the exact equivalent of that of Barrême.

These facts teach us that an ocean-bed existed, but that some coast-line, maybe only an interrupted line, once stretched across the present Atlantic, and permitted the Gosavian and Oligocene corals, and the Miocene shells also, to cross. I do not overlook the fact that Dr. Philippi himself, struck by the analogies existing between the flora of Chili and that of Europe, recently refused to accept the hypothesis of a "bridge" to Europe, and preferred to suppose that identical climatic and other external causes produced analogous and even identical species of terrestrial plants. I refer to what has been excellently said by Mr. Blanford on this theory, a few years ago, in his address to the Geological Society of London. I believe that the parallel correspondence of the marine faunas up to the Quaternary period gives a more correct clue to the correspondence of the existing terrestrial floras in Chili and in Europe.

So I think that we must not only concede the extinction of a great

[1] See Hyatt and Dillen on the Jura and Trias at Taylorville, California. *Bull. Geol. Soc. America*, 1892, pp. 369—412.

Palæozoic, Mesozoic, and Tertiary ocean in south-western Eurasia, but admit also great recent changes in the middle or southern Atlantic. Geological evidence, therefore, does not prove, nor even point to, a permanence of the great depths, at least in the oceans of the Atlantic type.

Let me remark in a few words that, although I believe in the possibility of the formation of large new depressions, I do not hold with the old opinion, lately taken up again by M. Faye, that the continued sinking of the ocean beds may force chains of mountains to appear all round. This view could only be propounded for the Pacific basin; but the Pacific chains are folded in the direction *towards* the ocean, and not *from* the ocean. They are easily divided into arches, each of which presents the *convex* side to the ocean, so that the Pacific everywhere presents the character of a "vorland."

Let me, at the end of this long note, allude to a broad biological fact. In the higher beings we see *lungs* always preceded by gills; so it is even with the human child. The adaptation for breathing our atmosphere is of a later date; and we conclude that the whole terrestrial air-breathing fauna is a *derived* fauna, derived from amphibious forms quitting shallow water. This fact evidently points to a long existence of dry land, long enough to permit this accommodation to be effected; the accommodation clearly has been going on since Palæozoic times. Still there exists no proof that individual continents always remained the same, and we even know for certain that such was not the case with by far the greater part of these continents.

A similar lesson is also taught by the *eyes* in all the higher organised beings of the deep sea. The optical apparatus of abyssal species is profoundly modified by the exceptional environment, while the normal types of eyes are met with in the same genera within moderate depths. Therefore, we must also regard these deep-sea forms as *derived* forms. The blind and blinded Trilobites of Cambrian beds, the blind Trinuclei and the widely-expanded eyes in certain species of Aeglina in Lower Silurian strata teach the same lesson. At the same time, they show that deep-water must have existed over Bohemia, and over a good many other Palæozoic tracts, and that the depths were considerable enough to call forth these same abyssal metamorphoses of the eye.

We might, therefore, rather be induced to infer that in Prepalæozoic times there may have existed a universal hydrosphere or panthalassa covering the whole of the planet. Only with the first appearance of dry land began the deposition of clastic sediments. The higher forms of life may have been developed in waters of moderate depth and may successively have spread to the sun-lit continents, and to the dark depths, while the slow elaboration of the existing inequalities of the terrestrial surface was going on.

But this elaboration is still in progress. I believe with Reyer,

Fisher, Jukes, and many others that the great depths are mostly covered with volcanic products, with lava and ashes forming immense plains and overlain eventually by the deposits of the abyss; but I see no reason why parts of the ocean or even of the dry land may not to-morrow sink to form new depths, or why we should believe that all the great ocean basins have been continuously covered by water since panthalassic times. So far as the Atlantic is concerned, there even exists some evidence to the contrary.

But all this is unripe fruit. Our scholars will some day know more than their masters do now; so let us patiently continue our work and remain friends.

EDW. SUESS.

III.

The Origin and Classification of Islands.

EVERY island has its history, and in the case of all but newly-formed volcanic islands or coral islets, every island has a double history, that of the island itself and that of its colonisation by the plants and animals which live upon it. The rocks of which an island consists will give us an insight into, though not always a complete knowledge of, its geological history; and a study of its living inhabitants will generally enable us to decide whether it has been colonised as an island or by direct former connection with a continent. Some biologists maintain that the fauna of an island will show whether it has ever been united to a continent or not, and this is the question which I propose to discuss in the present paper, because the answer to it involves some important inferences and conclusions.

There are many ways in which an island can be formed; it may be but a portion of a continent severed from the mainland by the erosive action of the sea; it may be the mountainous part of a country which has sunk beneath the ocean; it may have been thrown up from the floor of the ocean by volcanic action, or it may have been built up by the growth of reef-making corals. There are, however, only two ways in which an island can have been populated without the intervention of man; either it must once have been united to a continent and its inhabitants must be the descendants of those that then lived on that continent, or else its tenants must have been transported across the sea by the help of drift-wood, or by birds, or by winds and storms.

It is evident, therefore, that *in most cases* there is likely to be a certain relation between the geological structure of an island and the nature of its fauna and flora. Islands formed in the ocean, whether by direct upheaval, or by volcanic eruptions, or by coral growths, are not likely to possess a large assemblage of plants or of animals; they may be covered with vegetation, but the animals found on them must be the descendants of occasional waifs and strays. On the other hand, an island which has once been part of a continent will, if it remain large enough, continue to support a large number of animals, and these will generally include a certain number of Mammalia and Amphibia.

Islands have consequently been divided into two great classes—

oceanic and continental—which are defined by Dr. A. R. Wallace in the following terms :—

Oceanic islands are "of volcanic or coralline origin, usually far from continents, and always separated from them by very deep sea, entirely without indigenous land mammalia or amphibia, but with a fair number of birds and insects, and usually with some reptiles."

"Continental islands are always more varied in their geological formation, containing both ancient and recent stratified rocks. They are rarely very remote from a continent, and they always contain some land mammals and amphibia, as well as representatives of the other classes and orders in considerable variety."[1]

As general definitions framed for the purpose of describing the conditions which have governed and limited the geographical distribution of animals, these sentences are doubtless sufficiently accurate, especially as Dr. Wallace admits there are some islands which do not come very clearly under either of these categories; but he proceeds to lay down a *canon* the truth of which is by no means so apparent. He says :—"The total absence of warm-blooded terrestrial animals in an island otherwise well suited to maintain them, is held to prove that such island is no mere fragment of any existing or submerged continent, but one that has been actually produced in mid-ocean. It is true that if a continental island were to be completely submerged for a single day, and then again elevated, its higher terrestrial animals would be all destroyed, and if it were situated at a considerable distance from land, it would be reduced to the same zoological condition as an oceanic island; but such a complete submergence and re-elevation appears never to have taken place, for there is no single island on the globe which has the physical and geological features of a continental, combined with the zoological features of an oceanic island."

Seeing how little we yet know of the geology of distant islands, this is a statement which further knowledge may at any time disprove, and there is, even now, good reason to believe that it is contrary to facts. If this assertion can be proved to be incorrect, I shall claim to reverse Dr. Wallace's argument, and to maintain that inasmuch as an island does exist which combines the geological features of a continental island with the zoological features of an oceanic one, then we may assume that the submergence and re-elevation of a continental island can take place, and, consequently, the absence of mammals in an island *cannot* be held to prove that it has never been united to a continent.

Considering the many subsidences and upheavals which are known to have occurred along the borders of continental areas since the beginning of Tertiary time, it would indeed be strange if some tracts, isolated by subsidence, had not been completely submerged for a time, and afterwards raised afresh from the sea. The West Indian

[1] "Island Life," by A. R. Wallace, second edition, 1892, p. 243.

region is one where such an occurrence is very likely to have happened, for deep-water deposits of late Tertiary age occur in many of the islands; while the raised coral-reefs which are found in the same islands, and reach up to a height of 1,800 ft. above the sea, prove that there has been recent upheaval to at least that extent.

Many of the smaller islands are volcanic, and may have been thrown up at any time; but Barbados, the most westerly of all the islands, has just the features of which we are in search; in its faunal aspect it is decidedly oceanic, while its geological structure is a curious combination, being partly continental and partly oceanic. The facts of the case are so remarkable that a brief review of them may here be given.

Barbados stands on a submarine bank or ridge which slopes away in every direction till a depth of more than 1,000 fathoms is reached. The core and base of the island consists of stratified rocks, ordinary sandstones, clays, and limestones, such as are formed in shallow water near a coast-line where rivers of some size carry detritus into the sea, and these strata must have been deposited very near such a shore, for many of the sandstones are composed of large quartz grains, which would not be carried far from land. Above these shallow water strata lie deposits of a totally different character, consolidated oceanic oozes, like those which are now found only in the deeper parts of the ocean, and are known as Globigerina Ooze, Radiolarian Ooze, and Red Clay. All these kinds of ooze occur in Barbados, and there is not only a superficial resemblance between them and the modern oceanic oozes, but a complete identity of structure, and a close analogy in chemical composition; upheaval and exposure to rain and weather have, of course, effected some little alteration, but have not obscured their structure.

It is certain, therefore, that the shallow sea and the extensive shore-line which it bordered sank to a very great depth, certainly to more than 1,000 fathoms, and probably to as much as 2,000 fathoms (12,000 feet). The site of Barbados was then part of the ocean-floor, but after a time upheaval took place, and it was gradually raised till it came within the sphere of reef-building corals; a small coral islet was the result, but as the upheaval continued, the earliest reefs were raised above the sea, and the area of the island was gradually enlarged. This process went on till the island attained its present dimensions (about the size of the Isle of Wight), the soft oceanic deposits and the still older sandstones and clays being protected from the erosive action of the waves by a thick coating of coral rock, except over a small area in the north-west part of the island, where the rain has cut deep valleys through the stratified rocks, and by carrying sand and mud into the sea has prevented the growth of continuous coral reefs on that side.

Now, an island with such a history must necessarily have received its present fauna and flora in the same casual way as an

oceanic island that had never formed part of a continental area. Accordingly, though Barbados is exceedingly fertile, and though the island when first discovered was clothed with forest and underwood, its native terrestrial fauna is a very small one.

There are only two mammals in Barbados which have been supposed to be indigenous, a monkey and a racoon-like animal, but I am informed by Col. Fielden that the monkey proves to be the Green Monkey of Western Africa, *Cercopithecus callitrichus*, and the "racoon" is a South American animal (*Procyon cancrivorus*). The monkey was doubtless brought over in slave-ships, and as it is known that the Caribbean Indians frequented the island before it was colonised by Europeans, and as the early settlers had intercourse with the colonists of Guiana, it is quite possible that the Procyon was introduced by man.

There are no indigenous Amphibia, but there are two Snakes and four species of Lizards. One of the snakes is a species peculiar to Barbados, the other may have been introduced by human agency from some of the other islands. Of the lizards, three are South American species, and the fourth is found in all the Lesser Antilles, though it is not yet known from South America. The manner in which reptiles may be landed on an island like Barbados is illustrated by the case recorded by Col. H. W. Fielden in the "Zoologist" of 1888, p. 236; this was the landing of an alligator on the shore from a floating tree-trunk, actually witnessed in 1886; it had doubtless been transported from one of the great South American rivers, but it was promptly dispatched by those who witnessed its arrival.

In a paper on the birds of Barbados, Col. Fielden remarks that, so far as he can judge, "the mammals, reptiles, and land molluscs owe their introduction either to ocean-currents, accidental occurrences, or to the direct agency of man, and a review of its avi-fauna does not point to a different conclusion." He also speaks of Barbados as a "truly oceanic island in the sense of its never having formed part of a continent since the introduction of its present meagre fauna," nor "since it emerged as a coral-reef from the ocean." This is perfectly true, but yet it does not come under Wallace's definition of an oceanic island for the reason already stated.

The difficulty of drawing hard and fast lines between oceanic and continental islands is also illustrated by the structure and fauna of the Seychelles Archipelago, in the Indian Ocean. These islands are surrounded by water of more than 1,000 fathoms, and are 850 miles distant from the coast of Africa. They might, therefore, be expected to exhibit all the features of oceanic islands; the facts, however, are as follow: The larger islands consist entirely of granite, and granite is a deep-seated rock which can only be exposed by the prolonged and repeated processes of erosion which take place on large areas of land. Dr. Wallace admits them to be remnants of

"a very extensive island," though he doubts whether this island ever formed part of a continent.

Turning to their zoology, we find that they are entirely destitute of mammals, but that they possess Amphibia, having two species of frogs and three species of Cæcilia, snake-like creatures, which burrow underground in the manner of worms. Now, it seems impossible to explain the presence of these Amphibia unless, at some remote period, the islands formed part of a continent, for salt-water is fatal to them, and destroys even the eggs of frogs. On the other hand, if the former connection with a continent be admitted, what can be said of the absence of mammals, for one would have expected that some of the smaller genera, such as rats, mice, civets, lemurs, and insectivores would have survived. Dr. Wallace suggests that the islands have at some time been so nearly submerged that the portions remaining above water were too small to support the existence of the smallest mammals. If this be the explanation, and if we accept the evidence of the Amphibia as to ancient continental connection, then the absence of mammals in such islands cannot be taken as proof that they have never been part of continental land.

New Caledonia, again, in the Pacific, is regarded by Dr. Wallace as an oceanic island. There are no indigenous Mammalia or Amphibia, the solitary frog being known to have been introduced. There are several peculiar lizards and a snake (one of the Boas); and the island is separated from neighbouring groups by water of more than 1,000 fathoms deep. Notwithstanding this limited vertebrate fauna there is evidence that New Caledonia has once been part of an extensive land area. Stratified rocks, believed to be of Secondary and Tertiary age, enter into its geological structure, and there is a genus of land snails (*Placostylus*) which occurs in the neighbouring archipelagoes, as well as on Lord Howe's Island and in New Zealand. Hence it has been recently argued that the unity and limitation of the *Placostylus* area can only be explained by the supposition that these islands are portions of a broken-up and submerged continent which was distinct from Australia. To reconcile such a supposition with the absence of mammals, it is only necessary to assume that this continent dates back to a time when mammals were not in existence in the Pacific region, just as Australia dates from a time when only marsupial mammals were in existence.

The cases above mentioned show that there is no constant relation between the geological structure of oceanic islands and the manner in which they have received their present inhabitants, for there are islands which are not far from a continent, and have clearly formed part of the continental area within the limits of Tertiary time, and yet are without any indigenous Mammalia or Amphibia. There are other islands which are destitute of Mammalia and are geographically oceanic, but nevertheless, have a geological structure of continental type. In other words, there are islands which testify to the former

existence of continents in what are now oceanic areas; and when all the facts are considered it is seen that they are opposed to the extreme views regarding the permanence of oceans and continents which have been accepted and maintained by Dr. Wallace in his "Island Life."

It would appear, therefore, that if the division into continental and oceanic islands be retained, a fourth class must be admitted into the classification; not only must continental islands be divided into ancient and modern, but oceanic islands must be defined geographically and then divided into those which are the remnants of sunken continents and those which are of recent origin. It will, however, be very difficult to distinguish islands of recent formation from mountain peaks which have been submerged and again elevated above the ocean level, for lofty mountain peaks so often consist of volcanic rocks.

I am convinced that the attempt to exclude islands in which stratified rocks occur from the category of oceanic islands will only lead to confusion and misconception, and that it is a mistake to infer from "the absence of warm-blooded terrestrial animals in an island otherwise suited to maintain them," that the island is of recent formation. The characters of an island fauna may, perhaps, be relied on to show whether the island has been *colonised* by former connection with a continent or not, but beyond this it will be no guide to the geological history of the region. The biological evidence must simply be taken for what it is worth, and the geological history of the island must be read from its geological structure, without the bias given by any preconceived theoretical ideas about the permanence of oceans and continents. There is, in fact, no hard and fast line to be drawn between oceanic and continental islands.

A. J. JUKES-BROWNE.

II.

NOTE ON MR. JUKES-BROWNE'S PAPER.

The Editor having kindly sent me a proof of Mr. Jukes-Browne's paper, I beg to make a few remarks thereon.

I cannot but think that Mr. Jukes-Browne's criticism of the Darwinian classification of islands, which I have adopted and more fully developed, is rather one of words and definitions than of realities. The very terms of the classification—"Oceanic" and "Continental"—show that it is a broad and wide-reaching one; and its main implication, the permanence of oceanic and continental areas, is equally broad and fundamental. That there should be islands situated upon the ever-fluctuating margin of these two areas which are difficult to class, or which may, at different geological periods, have possessed the characteristics of "oceanic" or of "continental" islands, is what might certainly be expected; the wonder is that there are so very few of them. Barbados is, technically, an oceanic island; but it is

situated upon the old sea-margin of the American continent, and a portion of that old continental margin forms the base of its oceanic deposits. I recognised the possibility of such a base for some apparently oceanic islands in the passage quoted by Mr. Jukes-Browne, but was not then aware that any such existed. Of course, if old stratified rocks could be shown to form the base of any of the mid-oceanic islands, the whole classification, and the theory which is founded on it, might be imperilled; but this has not yet been done.

It is evident that, with island groups whose components vary in size from many thousands of square miles to small sea-washed rocks, all definitions must be taken broadly and as applying to the group. Even among the pre-eminently continental British Isles there are many hundreds—out of the thousand of which they are said to consist—which have neither mammalian, amphibian, nor reptilian inhabitants; but it will hardly be objected that such cases as these upset the biological definition of continental islands. In the same manner the Seychelles are classed as belonging to the Madagascar group, and are, therefore, ancient continental, while Mauritius, Bourbon, and Rodriguez are true oceanic islands.

I do not know why Mr. Jukes-Browne should say that I regard New Caledonia as an oceanic island. At p. 473 of *Island Life* I refer to it as probably once connected with New Zealand; and again, at p. 485, I suppose it to have once formed an extension of New Zealand, which, though in some respects anomalous, has all the main characteristics of a continental island.

Looking at the question broadly, as a generalisation applying to all the well-marked islands and island-groups of the globe, I entirely deny the validity of the conclusions expressed in the last three paragraphs of Mr. Jukes-Browne's paper, conclusions which are founded exclusively on islands situated upon the margin of the continental area.

ALFRED R. WALLACE.

IV.

Biological Theories.

III.—THE RECAPITULATION THEORY.

DIRECT observation has shown that, when an animal species *varies* (*i.e.*, *becomes* unlike what it was before) in adult structure, those stages of the development which are nearest the adult undergo a similar but usually smaller change. This is shown in domestic species by the observations of Darwin, and the result is in exact harmony with the well-known law of Von Baer, which refers to natural species, both nearly related and very widely dissimilar.

Von Baer's observations as well as Darwin's, and as well as those of every student who has ever compared the embryos of two vertebrate-species, may be summarised as follows:—

Animals which, though related, are very unlike in the adult state, resemble each other more closely in early stages of development, often, indeed, so closely as to be indistinguishable in those early stages. As development proceeds, *in such species*, the differences between the two embryos compared become more and more pronounced.

If similar comparisons could be instituted between an ancestral species and its much modified descendants, there is no reason for doubting that a similar result would be reached. This, indeed, has been done in the case of some breeds of pigeons, which we have excellent reasons for believing to be descended from *Columba livia*. True, *C. livia* is not a very remote ancestor, but I do not think that will vitiate the argument. Let me quote Darwin verbatim: "As we have conclusive evidence that the breeds of the Pigeon are descended from a single wild species, I have compared the young within twelve hours after being hatched; I carefully measured the proportions (but will not here give the details) of the beak, width of mouth, length of nostril and of eyelid, size of feet, and length of leg in the wild parent-species, in pouters, fantails, runts, barbs, dragons, carriers, and tumblers. Now some of these birds, when mature, differ in so extraordinary a manner in the length and form of beak, and in other characters, that they would certainly have been ranked as distinct genera if found in a state of nature. But when the nestling birds of

these several breeds were placed in a row, though most of them could just be distinguished, the proportional differences in the above specified points were incomparably less than in the full-grown birds. Some characteristic points of difference—for instance, that of the width of the mouth—could hardly be detected in the young. But there was one remarkable exception to this rule, for the young of the short-faced tumbler differed from the young of the wild rock pigeon and of the other breeds in almost exactly the same proportions as in the adult state." (" Origin of Species," 6th edition, p. 392.)

As, it seems to me, would be expected, the differences between two more-distantly related animals at a stage corresponding to the time of hatching in pigeons is very much greater. A bird is obviously a bird at the time of hatching, and often the species and even the breed (as in the case just quoted from Darwin) are readily recognised at that time. A lizard or other reptile at the corresponding stage is easily recognised as a reptile. As shown by Von Baer the distinction between the bird and the reptile becomes more and more difficult as we examine earlier and earlier stages, and in the earliest stages, *i.e.*, in the ovum, it is often quite impossible to say even to which great phylum of the animal kingdom the ovum belongs.

The variation which produced birds from reptilian ancestors may be taken as a type of the kind of variation which is most familiar to us, *i.e.*, the variation affecting chiefly the adult animal.

Another kind of variation is illustrated by the three genera of gnats, *Culex*, *Corethra*, and *Chironomus*. Whatever may have been the form and structure of the most recent common ancestor of these three, the variation which has produced these from that ancestor appears to have affected the larval stages more markedly than the adult stages. These three are quite unlike in appearance in the larval stage. The mode of formation of the imago within the larva in *Chironomus* is quite unlike that in *Culex*. The three larvæ differ not only in appearance, but in internal structure. In the adult stage, though easily distinguishable, they are very much more alike in form, and even in internal structure, than in the larval stages.

We may not know the exact course of evolution of these three, but we may at least say that the variation in the average structure which has occurred in their evolution has led to a greater difference in larval structure than in adult structure.

Two kinds of variation must, therefore, be recognised—that affecting chiefly the adult structure, and that affecting chiefly the structure of the individual in early stages of development.

My object now is to show that in neither case can a record of the variation at any one stage of evolution be preserved in the ontogeny, much less can the ontogeny come to be a series of stages representing, in proper chronological order, some of the stages of adult structure which have been passed through in the course of evolution.

This would seem to be obvious enough, and there would appear to be no need for any argument to establish the view here put forward, viz.:—that the ontogeny is not an epitome of the phylogeny, is not even a modified or "falsified" epitome, is not a record, either perfect or imperfect, of past history, is not a recapitulation of the course of evolution. My excuse for urging what appears to me to be an obvious fact, is the existence of a statement the direct opposite of this view in almost every zoological text-book which has been published within the last fifteen or twenty years, as well as the frequent urging of the recapitulation theory in our colleges and universities, in the University Extension Lectures, and in a recent lecture given to the British Association, and even in the presidential address of one of our most eminent embryologists to the Biological Section of that Association.

Incidentally, it may be well to point out the restricted sense in which I use the word "variation." To vary, is, I believe, to *change* or to *become unlike*, whatever the thing varying was previously like. To *be unlike* is *to differ*; *not to vary*. The unlikeness observable among the members of a species is variety, or difference, or unlikeness, but it is not variation. Variation is change in the average constitution of successive generations of a species leading to the production of a new species, or race, from an old one. I have myself used the term in another sense in a previous paper in this series, but that is no reason why I should continue to use it in a sense which is liable to lead, and indeed has led, to confusion.

The terms "vary" and "differ" as above defined are not even partially synonymous. Difference and variation are respectively statical and dynamical (if I may use that term to express the opposite of statical).

In order that any structure of the adult which varies, and hence ceases to exist as an adult structure at all, may become an ontogenetic record of that adult structure, it is necessary that variation should occur in a way utterly unlike the way in which it does actually occur. The more the adult structure comes to be unlike the adult structure of the ancestors, the more do the late stages of development undergo a modification of the same kind. This is not mere dogma, but is a simple paraphrase of Von Baer's law. It is proved true, not only by the observations of Von Baer and of Darwin, already referred to, but by the direct observation of everyone who takes the trouble to compare the embryos of any two vertebrates, provided only he will be content to see what actually lies before him, and not the phantasms which the recapitulation theory may have printed on his imagination.

In order to produce a "record" it is necessary that new chapters be added at the end of the pre-existing record. It is necessary, in fact, that as the adult structure varies in one direction, the late stages of development shall vary in another, so as to become, not more like the new adult structure than they were before, but more like the old

one. If the variation is a mere increase in the definitive size of an organ which has hitherto not only increased in size in the evolution of the species, but also has *grown* during development of the individual, then, of course, a variation affecting the rudiment of this organ at all stages of development in which it is present, and causing it to grow more rapidly, will render the late stages of development (as far as that organ is concerned) more like the adult stage of the ancestor. This, however, is a mere result of the mode of growth of the particular organ ; its smaller size in late embryonic stages than in the adult is not a result, still less is it a "record" of the course of evolution.

The antlers of stags will serve as an example. Each stag develops a new pair of antlers in each successive year, and each pair of antlers is larger than the pair produced in the previous year. This yearly increase in the size of the antlers has been put forward as an example of an ontogenetic record of past evolution. I, however, deny that it is such a record. The series of ancestors may have possessed larger antlers in each generation than in the generation before it. It is not an occasional accidental parallelism between the ontogeny and the phylogeny which I deny, but the causal relation between the two. Had the ancestors had larger antlers than the existing ones, there is no justification for the assumption that existing stags would acquire antlers of which each pair, in later years, would be smaller than those of the previous year. The yearly increase in the size of the antlers is itself a character determined by Natural Selection. Phylogeny appears to have run parallel with the *pre-existing* Ontogeny. There are many breeds of hornless sheep, but they do not bear large horns in early years and then shed them. If a rudiment ever appears in the embryo of such sheep, its growth is very early arrested.

So it is in all the alleged cases of recapitulation. The gill-arches and clefts, and the blood-vessels of an embryo bird or mammal, present that striking resemblance to the corresponding parts of the *embryo* of a fish which is expressed in Von Baer's law. They differ, perhaps, only very slightly indeed. They are the modified representatives of the embryonic structures of a common ancestor. Whether they were, in that ancestor, the rudiments of gill-arches and clefts, &c., like those of an adult fish, or not, cannot be decided by embryological study. All we learn is that what now serve in their modified forms as the rudiments of the gill-arches of a fish, and of certain parts about the throat of a bird or a mammal, are so similar in early stages of development as to show that these parts are homologous. The greater resemblance of them to the adult structures of a fish than to those of a mammal may justify the belief that they served in a common ancestor as the rudiments of adult structures more like the adult structures of a fish than of a mammal, and that is all: they do not prove even that.

The statement that this stage in the development of a bird or a mammal is the modified remnant of the *adult* structure of the ancestor; and that the ontogeny is even in part made up of a series of remnants of ancestral *adult* structures arranged in chronological order, is not only unjustified, but is demonstrably false. If any generalisation in the whole science of zoology is borne out by fact, it is the law of Von Baer with reference to animals living free only in later stages of development. That law claims a parallelism between the development of a fish and of a bird which is quite inconsistent with the recapitulation theory, and completely consistent with the observed facts.

The early stages of the fish embryo are very like those of the bird embryo. These two *do* correspond to each other. The statement that the embryonic structure of a bird follows a course which is, from beginning to end, roughly parallel with, but somewhat divergent from, the course followed by a fish, is borne out by the actual facts. A bird does not develop into a fish and then into a reptile and then into a bird. There is no fish stage, no reptile stage, in its ontogeny. The adult resembles an adult fish only very remotely. Every earlier stage resembles the *corresponding earlier stage* of the fish more closely. There is a parallelism between the two ontogenies. There is no parallelism between the ontogeny and the phylogeny of either a bird or any other animal whatever. A seeming parallelism will fall through when closely examined.

Each transient stage in the development of any individual is a modification of the corresponding stage of development of its ancestors. It is in no case a modification of the adult stage of the ancestor. The adult stage of a bird, and no other, corresponds to the adult stage of the fish-like ancestors (if it ever had such ancestors).

The stalked "pentacrinoid" larva of *Antedon* (=*Comatula*) is a modified equivalent of the stalked *larva* of the "pentacrinoid" ancestor (if ever there was such an ancestor), and not the modified equivalent of the adult ancestor. The possession of a stalk in early stages of development appears to be an advantage, and hence the specific constitution which determines the development through a stalked stage has been preserved by Natural Selection. There is no evidence whatever to justify such mystical conceptions as those involved in even the most reasonable forms in which the recapitulation theory has been applied to this case.

The promise which the theory gave of serving as the guide to knowledge of past history, without the labour involved in palæontological research, was, indeed, tempting: and when the "royal road to learning" had been shown by it, it is not surprising that some zoologists should have entered for the race along this road. To what goal that road has led may be learned by a comparison of the numerous theories as to the ancestry of "chordata" which have been put forward by those who adopted the theory without enquiring as to its validity.

In this connection it may be not out of place to conclude with a quotation from Fritz Müller :—

" A false conception, when the consequences proceeding from it are followed further and further, will sooner or later lead to absurdities and palpable contradictions." (Opening sentence of chapter ii., " Facts for Darwin.")

That is precisely what the "recapitulation theory" has actually led to.

C. Herbert Hurst.

V.

Recent Observations on Fertilisation and Hybridity in Plants.

THE object of the following paper is to bring before the readers of NATURAL SCIENCE the most recent results which have been attained by workers in that department of Plant Physiology which relates to the actual process of the fertilisation of the female by the male element, and of the secondary processes by which the access of the active to the passive element is assisted. The enquiry naturally divides itself into two branches: (1) The nature of the process itself; (2) The subsidiary phenomena. The first of these enquiries evidently goes to the foundation of the laws on which depends the succession of life on the earth. Observations on this head, to have any value, must be carried out by experts of great knowledge, and with trained skill in the use of the most delicate microscopical appliances; the lowest forms of life, as well as the highest, must be put under requisition to yield up their secrets. As we ascend in the scale of organised beings, the various vital processes become more complicated, and secondary phenomena play a larger part in them; and it is in Flowering Plants that these become most interesting and most within the scope of the non-scientific observer. As, therefore, this will be the most familiar branch of the subject to those of our readers who have not made the physiology of plants their special study, we propose to commence with it.

The process by which the ovule of Flowering Plants is fertilised by the pollen-grain—*i.e.*, is acted on so as to be enabled to produce an embryo, without which no seed can germinate—is sufficiently well-known. The pollen-grain (the male element) must first be deposited on the stigma, where it puts out a pollen-tube which penetrates into the embryo-sac of the ovule, and there, coming into contact with one of the embryonic vesicles (the female element) fertilises it, the result being the growth of the embryo within the embryo-sac. The subsidiary phenomena are here the conveyance of the pollen-grain to the stigma, and the entrance of the pollen-tube into the embryo-sac. In the last-named process a great uniformity prevails throughout the Angiosperms or larger section of Flowering Plants; the only important exception being the

very interesting one recently discovered by Treub in the case of the Casuarineæ; but as that has already been fully described in these columns,[1] we will not further allude to it. Much greater variation exists in the phenomena connected with pollination, or the carriage of the pollen-grains to the stigma.

There is no more fertile source of error than the too rigid or too universal application of general laws. It is perfectly true that Nature "works not by partial, but by general laws." But a natural law is not, as some seem to suppose, an external force which directs the phenomena of life, it is simply a generalising of the phenomena themselves. A certain course of proceeding is of advantage to a particular organism or set of organisms; in other words, they are governed by a general law; but in some special case the same end is best attained by other means, and we find what is generally termed an exception to the general law. In no department of physiological botany is this variation more strikingly exhibited than in the phenomena of pollination. It was in 1793 that Christian Konrad Sprengel's celebrated work *Das entdeckte Geheimniss der Natur im Bau und in der Befruchtung der Blumen* was published. Centenaries have been celebrated on more trivial grounds than the appearance of this "epoch-making" work; since in it was first clearly laid down the law that in a large number of flowers the structure and arrangement of the male and female organs render self-pollination almost impossible, understanding by this term the pollination of the stigma by pollen-grains from anthers in the same flower. It is a matter of general knowledge how greatly this law was exemplified and extended by Darwin and his followers; but here again the tendency to a too universal application of the law has manifested itself, and during the last few years a remarkable number of observations have been made which indicate that self-pollination is far more general than had at one time been supposed. The perfume and the bright colour of flowers are undoubtedly important agents in attracting insects to assist in cross-pollination; and it has been assumed that this must always be their function. But that this is not invariably the case is certain. It has long ago been pointed out that the bee-orchis, *Ophrys apifera*, the flower of which so remarkably simulates a bee, is not visited by insects, and yet produces seeds abundantly. Very curious also are the facts in connection with the genus *Aristolochia*, in which the conspicuous pitcher-shaped perianth has been regarded as a contrivance for attracting insects and ensuring self-pollination. The walls of the pitcher are clothed with woolly or glutinous hairs, which have been assumed to have the function of detaining the pollen brought by the insects which enter the pitchers in great numbers from other flowers, and thus enable them to reach the stigma. W. Burck states, however, as the result of observations

[1] NATURAL SCIENCE, vol. i., p. 132.

on several species of *Aristolochia*, that when flies enter the chamber formed by the lower part of the perianth, and become dusted with pollen, in their efforts to escape again from the chamber they come into contact with these hairs, and thus lose, before they pass to another flower, almost every grain of pollen. In *A. barbata* at least 600, and in *A. ornithocephala* about 6,000 pollen-grains would be required for the fertilisation of all the ovules in an ovary. Out of a very large number of flies captured in these chambers, only a very few showed the presence of even a small number of pollen-grains adhering to them. On the other hand, both these two species and *A. elegans* are abundantly fertile when pollinated with their own pollen. He concludes, therefore, that these species at least are self-pollinated. Herr Burck also points out the interesting fact that in *Aristolochia* the true stigma and style are abortive, and that the so-called stigmatic surface consists of the connectives of the anthers which have coalesced by their sides into a cup, are provided on their margins with papillæ, and have assumed the functions of a stigma. In the instances just named, and in others of the same kind, it is an allowable hypothesis that the bright colour or sweet scent of the flower is a survival from cross-pollinated ancestors which has now lost its meaning. But this will not apply in other instances. The hazel and the larch have, it is true, unisexual and therefore necessarily cross-pollinated flowers. But they are not visited by insects, and are universally regarded as typical examples of wind-fertilised plants. What, then, is the object of the bright red colour of the styles of the one and of the scales of the opening cones of the other? It cannot be a survival, since the Corylaceæ and the Coniferæ are both unquestionably archaic forms of life. Bright colours, indeed, appeared very early in the evolution of plant-life. How are we to explain the brilliant red of the sporange of *Sphagnum*; or the bright pigment of the antherid or "globule" of the Characeæ; or the brilliant colour of species of *Peziza* or *Boletus*? Does Nature love beauty for its own sake?

Prominent among those who advocate the anti-Darwinian view of the prevalence of self-pollination, is Mr. Thos. Meehan, of Philadelphia, a botanist of great experience in the cultivation of plants. He adduces a large number of American plants in which he asserts this mode of pollination to be the rule, among others, in *Amsonia Tabernæmontana*, belonging to the Apocynaceæ, in which the flowers are showy and abundantly fertile, but their structure is such that no insect, not even a thrips, can gain entrance to the nectary. The mouth of the corolla-tube is so densely matted with hairs that if the pollen-clothed tongue of an insect were thrust through the mass, it would be completely cleansed; nor is there any room for the tongue to pass the capitate stigma. To effect pollination the anthers curve over and rest upon the stigma. Other examples of habitual self-pollination are given in *Symplocarpus fœtidus*, belonging to the

Aroideæ, in which the flowers are frequently proterogynous and as frequently proterandrous; in *Portulaca pilosa*, the flowers of which open only in the sunshine, and yet seed abundantly when grown in the shade; in *Cuphea Zimpani* (Lythraceæ), *Lopezia coronata* (Onagraceæ), in several species of *Lonicera*, *Phytolacca decandra*, *Lycopersicon esculentum*, *Hamamelis virginica*, &c. A large number of the American Compositæ he asserts to be self-pollinated. He has observed pollen-tubes entering the clefts of the bilobed stigma before it opens; and the pollen-grains, even when large and brightly coloured, frequently fall on the stigma before any insect can possibly enter the flower. Mr. Meehan, indeed, goes so far as to say that whenever a species is unusually productive, he finds, as a rule, arrangements for self-pollination. An Italian botanist, Dr. Terraciano, states that in many species of *Nigella*, notwithstanding the conspicuousness of the flower and the presence of nectaries, the structure is adapted for self-pollination. According to Warming, all the Greenland and Iceland species of *Euphrasia* are self-pollinated. Even so decided a Darwinian as Professor Caruel, of Florence, is inclined to doubt whether too much has not been taken for granted in the prevalent theory of the part played by the bright colour and sweet scent of flowers in attracting insects, seeing that, as far as we know, the visual and olfactory organs and perceptions of many animals are very different from our own. In particular, he calls attention to Sir John Lubbock's observations on the different effect of colours on many animals from that which they produce on us; to the eyes formed of facets and to the ocelli of insects, and to their sensitiveness to the ultra-violet rays of the solar spectrum. Rosen asserts that even with many wind-pollinated plants, such as *Carex* and *Festuca*, self-pollination is the rule.

On the other hand, the orthodox view receives support from a very large nnmber of fresh observations which reveal adaptations clearly intended to promote cross-pollination by the agency of insects. In a series of papers in the *Botanical Gazette* and elsewhere, Mr. C. Robertson describes a large number of observations to this effect on American plants. Professor Halsted states that the flowers of the barberry are very rarely, if ever, self-pollinated, and calls attention to the remarkable irritability of the stamens of species of *Portulaca*, which promotes the scattering of the pollen over the bodies of insects visiting the flowers. The structure of the flowers of *Arum*, *Dracunculus*, *Helicodiceros*, *Arisæma*, *Amorphophallus*, and other species of Aroideæ, has been made a special study by several Italian botanists. The extraordinary simulation by the open flower of the appearance and odour of decomposing flesh, appears specially designed to deceive and attract necrophagous coleoptera and diptera, which aid in the carriage of pollen. Schulz asserts that in the family Sileneæ of Caryophyllaceæ the proterandry is in many cases so marked as to render self-pollination impossible. Mr. G. F. Scott Elliot has, in the *Annals of Botany*, a very interesting paper on the

pollination of South African plants by birds. The Cinnyridæ, or sunbirds, are exceedingly good pollinators, especially *Nectarinia chalybea* and *bicollaris*, and *Promerops caper*. Like bees, they, as a rule, visit only one species of flower at a time. Mr. Scott Elliot believes that the identity of colour—an unusual shade of red—in the majority of ornithophilous flowers and on the breast of species of *Cinnyris*, is an important element in this pollination. Mr. H. N. Ridley calls attention to a remarkable structure in the flowers of *Bulbophyllum macranthum* and other orchids of Singapore for cross-pollination by the agency of insects. C. Correns describes the special arrangements for this purpose in *Salvia* and *Calceolaria*. Lists of cross-pollinated plants are given by McLeod from Belgium and the Pyrenees, by E. Loew and Knuth from Germany, by Delpino, from Italy, and by Scott Elliot from Madagascar. Kellgren enumerates 33 species of Leguminosæ growing on the Omberg, an isolated mountainous region in Germany, chiefly covered with pine woods, all pollinated by lepidoptera. The mode of pollination in the different species of *Yucca* is of great interest, and has long attracted the attention of botanists. Professor Riley, who has made it a subject of special study, states that in all the species examined by him, self-pollination is almost impossible; and that in each species the pollination is effected by a single species of insect. The pollinator of *Yucca filamentosa* is a moth, the female of *Pronuba yuccasella*. The object of the visit of the moth is the deposition of its eggs in the ovules of the *Yucca*, the coats of which it pierces. This is always effected as soon as the flower opens; and the moment an egg has been deposited the insect rushes to the anthers and carries a quantity of pollen to the stigma, stuffing it into the stigmatic cavity with its proboscis. Ten or twelve ovules may thus be destroyed, but the number in each ovary is so large that this does not practically affect its fertility. In Philadelphia, where the moth makes its appearance about the time that the *Yucca* is in flower, the latter produces abundance of seed, while it does not set its fruit in Washington and St. Louis, where it flowers a fortnight before the arrival of the moth. Finally, the report, in the *Botanical Gazette*, of a series of experiments carried on by Miss M. Reed, mostly on petunias, fully confirms Darwin's statement that cross-pollinated produce more seed-vessels than self-pollinated plants, and that the capsules are heavier.

Attention has long been called to the fact that not a very small number of plants produce, in addition to their ordinary showy flowers, others—termed cleistogamic—which never open, in which the corolla is partially or entirely suppressed, but which ripen abundance of seeds, being, of course, self-pollinated. A very good example of these cleistogamic flowers is furnished by the various species of "dog" violet, *Viola sylvatica* and its allies. The conspicuous flowers which appear in the early spring are mostly sterile;

but throughout the later spring and summer very inconspicuous flowers are abundantly produced, completely concealed in their calyx, and these almost invariably give rise to fertile capsules. It would appear as if nature provided in them a surety for the production of seed in case of the continued sterility of the early spring flowers. Several species of Leguminosæ produce cleistogamic flowers, borne, in some cases, on underground shoots, the seed-vessel never appearing above the surface of the soil, and other instances are furnished by the genera *Oxalis*, *Impatiens*, and many others. During the last few years considerable additions have been made, by Meehan and others, to the list of cleistogamic species, and these include several plants belonging to the British flora, such as *Polygonum acre*, *Hydropiper*, *Persicaria*, and *maritimum*, *Scleranthus annuus*, &c. In *Polygonum acre* and *Hydropiper* the cleistogamic flowers are very numerous, and appear always to produce ripe seeds; they are small and completely hidden in the leaf-sheaths. W. Burck describes several species of tropical plants which bear flowers that never open, and must, therefore, be self-pollinated, although coloured and scented, and producing abundance of nectar. This is strikingly the case with *Myrmecodia*, one of the genera which furnish abodes in their stems for colonies of ants; and he suggests the explanation that the flowers were at first adapted for cross-pollination, but that the visits of insects have been gradually suspended in consequence of the attacks of the ants which inhabit the tubers. A. Schulz connects the appearance of cleistogamic flowers with unfavourable climatal conditions. In *Tephrosia heterantha*, a native of the Argentine Republic, Hieronymus states that the pollen-grains are few in number in the cleistogamic flowers, and that their tubes pierce the wall of the anthers in order to reach the stigma.

The structure of the flowers in the genus *Vitis* is very interesting. In the native state all species of vine have two kinds of flower,—male, in which the pistil is subject to all degrees of abortion, and hermaphrodite, in which both stamens and pistil are fully developed. The stamens differ remarkably in the two kinds of flower. In the hermaphrodite flowers the filaments are short and curved backwards, so as to remove the anther as far as possible from the stigma; in the male they are longer and erect; but in the cultivated vines of Europe, all of which are varieties of *Vitis vinifera*, and have only hermaphrodite flowers, the filaments are long and erect, as in the male flowers of the wild plant. The pollen-grains of the two kinds of stamen also differ in their power of fertilisation. Millardet, who has given great attention to the phenomena connected with the fertilisation of the vine, states that in the wild state the vine is wind-fertilised, although the flowers have a powerful odour, the purpose of which is obscure. In the cultivated state he has observed abundance of two small coleoptera in the flowers, which may also probably take some part in the pollination. Kronfeld

states that the cultivated grape-vine is also occasionally pollinated by honey-bees.

As has already been pointed out, those flowers which are unisexual—*i.e.*, which have either pistil and no stamens, or stamens and no pistil—must necessarily be cross-pollinated. Schulz, however, states that in many of our trees which are normally unisexual, hermaphrodite flowers do occasionally occur. Thus, in the elder, hermaphrodite flowers or transitional forms between these and unisexual are frequently to be found at the base of the male catkins. Hermaphrodite flowers also occur in the birch, though less frequently, very rarely in the hazel; in the oak there are often ovaries at the base of the male catkins, and rudiments of stamens in the female flowers. The mode of fertilisation of the hazel is involved in great obscurity, since scarcely a trace of the ovules can be found in the female flowers at the time when the catkins are discharging their pollen. In the ash, all kinds of condition may be met with, male, female, and hermaphrodite flowers; and either the same or different kinds on the same tree. The ash is probably on the road to becoming completely diœcious.

Few phenomena in physiology, whether animal or vegetable, are more puzzling than those of parthenogenesis, that is, the production of a normal fertile embryo without any preceding act of fertilisation. The example in the vegetable kingdom of *Cælebogyne* is familiar to all students of text-books. Another very remarkable instance is now on record. There have been few more interesting contributions to botanical science during the last few years than Dr. D. D. Cunningham's "On the Phenomena of Fertilisation in *Ficus Roxburghii*," published in Calcutta in 1889, with beautiful illustrations. According to Dr. Cunningham the figs are in this species either male or female, all on the same plant being of the same sex. The male figs contain perfect male flowers, which produce pollen, and atrophied female or "gall-flowers," which never produce seed, but within the ovaries of which the eggs of an insect—usually a species of *Eupristis*—are deposited and develop into pupæ. The female figs contain perfect female flowers, in which the eggs of the insect are never found, and which produce fertile seeds. The terminal opening of both the male and female figs is so obstructed by a covering of bracts that they are almost completely closed chambers; and the perfect development of both the male and female flowers is dependent on the access of the "fig-insect" to the interior of the cavity, without which they do not arrive at a functional condition. Although the development of the embryo in the female fig is essentially connected with the access of the insects to the cavity, Dr. Cunningham believes that it does not depend on the introduction of pollen by their agency. The nearly entire closure of the opening by bracts presents an almost insuperable obstacle to the introduction into the female fig of a sufficient quantity of pollen for the fertilisation of every one of the very numerous ovules

by a separate pollen-grain; and but very few pollen-grains could be found in the female figs. Although it is possible that in some cases ordinary pollination may occur, the author asserts that the embryo is usually formed without any process of fertilisation, arising as an outgrowth of the embryo-sac. The full development of both the male and female flowers appears to depend on a simple hypertrophy of the tissues of the fig, resulting from the irritation caused by the female insect in the act of laying its eggs within the ovary of the "gall-flowers" of the male figs, and of their persistent attempts to do the same within the flowers of the female figs, in which attempts they are frustrated by the great thickness of the wall of the ovary. In connection with the fertilisation of the fig, it may be mentioned that Professor Riley has enumerated fourteen species of insect as taking part in the so-called "caprification" of the wild figs of North America. He recommends to the fig-growers of California the introduction for this purpose of *Blastophaga psenes*.

With regard to the connection between the duration of life of the individual and the mode of fertilisation, Meehan makes the general statement that flowers which are wholly dependent on insects for their fertilisation are always perennials, and that an innumerable number of their flowers fall unfertilised; while all annuals, on the other hand, can self-pollinate themselves when cross-pollination fails; and in almost all cases all the flowers of annuals are fertile.

The phenomena of hybridity in the vegetable closely resembles those in the animal kingdom. It is possible to fertilise an ovule by pollen belonging to a different species, but only if the two species are very nearly allied. It is very rarely that species which are fertile with one another can be placed in different genera. Hybridisation is one of the every-day resources of the gardener; but that cross-breeding occurs in nature has been doubted by some. There seems, however, scarcely to be room for doubt that in some of our abundant wild genera, such as *Rubus*, *Salix*, and *Hieracium*, hybridity is not uncommon in nature. It has long been known that in some genera, such as *Passiflora*, and in some Orchideæ, the ovules appear to be even more readily fertilised by pollen of a different species. W. Focke now states that this is also the case with the species of *Lilium* belonging to the group *bulbiferum*, and with some species of *Hemerocallis*; and J. H. Wilson affirms the same respecting the Cape genus *Albuca*, also belonging to the Liliaceæ. According to Millardet none of the so-called hybrid vines cultivated in Europe are true hybrids, *i.e.*, products of the crossing of distinct species; they all spring from the crossing of different races of the same species, *Vitis vinifera*. He further states that, in the vine, it is the male parent that exercises the preponderating influence on the descendants. Rimpau has carried out a series of experiments on the crossing of some of our most common agricultural plants. He has obtained ten artificial and nine natural hybrids in wheat, and also obtained a fertile hybrid between

wheat and rye belonging to different genera. Between different kinds of barley he obtained two artificial and six natural hybrids; in no case did he succeed in fertilising a two-rowed barley by pollen of a variety with a larger number of rows. Among oats, five natural but no artificial hybrids were obtained. Very few natural hybrids occur among peas; with different varieties of beet crossing is much more common. Rimpau states that if a new form exhibits great variability among its descendants, it is probably hybrid; while, on the other hand, it is most likely a spontaneous variety if the descendants maintain great constancy.

The Darwinian hypothesis that a sexual mode of reproduction is absolutely necessary to the maintenance of the higher forms of life, and that continual propagation by non-sexual methods must result in deterioration and ultimate extinction, has not been allowed to pass unchallenged during recent years. Möbius in Germany and Meehan in America have pointed out the great length of time, extending in some cases to thousands of years, during which some plants have been continuously propagated by non-sexual methods without apparent deterioration or increased liability to disease. This is the case with many fruit-trees, such as the fig, date-palm, banana, yam, batatas, and olive, and with the Canadian water-weed, *Elodea canadensis*, imported into this country from America, the male plant of which is still unknown here; though it is asserted by others that this pest is now gradually dying out from our rivers and canals. On the other hand, the weeping-willow and the Lombardy poplar—varieties which never produce seed, and which are propagated solely by cuttings—do appear to be threatened with extinction, owing to their abnormal liability to disease.

We now come to the second part of our subject, and, as we have already said, the more difficult one—the nature of the process of fertilisation itself. In general terms, fertilisation, or fecundation, may be defined as the union of the elements of an active male with those of a passive female cell, the result being the production of an embryo which develops into a new individual. In the animal kingdom, the only mode of reproduction in all the higher forms is a sexual one, non-sexual propagation having survived only in the lower types. In the vegetable kingdom, the two modes work side by side in the higher forms, while in the lowest forms sexuality is unknown. It becomes, therefore, a problem of great interest to determine in what way the sexual mode of reproduction arose among plants. Much light is thrown on this question by the phenomena which have been observed among Algæ. A very common mode of propagation among our ordinary fresh-water Algæ is by means of non-sexual zoospores or swarmspores, minute flagellate bodies which escape from the cells of the parent plant, move about in the water with great rapidity, and then finally come to rest, withdraw their cilia, and develop into new plants. Among the brown sea-weeds, there is a great variety in the

nature of the reproductive organs: we have non-sexual flagellate zoospores; we have a rudimentary sexual process in the conjugation of zoogametes, flagellated motile bodies which closely resemble zoospores, but which have no power of germination without first uniting in pairs; and, finally, we have an advanced mode of sexual reproduction in which the male and female elements differ almost as widely as they do in the highest flowerless plants, the former being a minute motile multiflagellate antherozoid, the latter a much larger perfectly passive oosphere. But what is most interesting is that we find in the brown sea-weeds all sorts of intermediate conditions between these, leading us to the irresistible conclusion that all the higher developments of sexuality have had their first origin in the union of two motile flagellate masses of protoplasm between which there is no apparent differentiation; but that very early, as we ascend in the evolutionary scale, a differentiation sprang up between these two masses of protoplasm, which became gradually more and more marked as they developed into what we call male and female cells. The latter soon lost their flagellate character, became quiescent, and increased in size; the former gradually passed from a biflagellate zoogamete indistinguishable from a zoospore, to the multiflagellate antherozoid of the higher Cryptogams. The passage from these to the germinal vesicles in the ovule and the pollen-tube of flowering plants is somewhat more difficult to follow.

The production or non-production of sexual organs in plants is very much a question of external conditions. In a recent paper in the *Biologisches Centralblatt*, Professor Möbius has published a very interesting summary of our knowledge on this subject. An abundant supply of nutriment promotes the production of vegetative rather than of reproductive organs; hence the value of root-pruning in increasing the fertility of fruit trees. The conditions which, on the whole, favour the formation and fertility of the sexual organs are—a high temperature, with not too much moisture and not too much supply of nutriment, abundance of light, and, in the case of the fruit trees of temperate climates, a period of complete rest in the winter. Many of the fruit-trees of our climate will not flower in the tropics. The potato, which blossoms only sparsely with us, produces flowers every year in its drier native country of Chili. *Epilobium angustifolium* will flower with us only in sunny situations, and the brighter the light the deeper the colour of the flowers. The production of flowers and fruit has an exhausting effect on the plant. A well-known illustration of this is the "century plant," *Agave americana*, which flowers only once, when from 30 to 100 years old, and then dies. A very abundant fruit year is commonly followed by one of comparative scarcity.

A similar law prevails also in the lower forms of vegetable life; but with this difference, that we find there the much greater plasticity which is the great feature of a low type of life. Many of our

commonest Algæ will fructify only in conditions which are unfavourable to the production of their vegetative organs. From an exceedingly interesting series of observations made by Klebs on that beautiful organism the "water net," *Hydrodictyon reticulatum*, we learn that all the cells in a net are apparently equivalent, *i.e.*, are adapted to produce either non-sexual zoospores or sexual zoogametes, and that the tendency to produce one or other of these structures is largely a question of nutrition. In a single net, consisting of equivalent sister-cells, some of the cells can be excited, by external conditions, to develop zoospores, others to produce zoogametes, the formation of the former being, in this case, absolutely dependent on light. A similar law prevails in *Volvox*.

Professor Weismann, in his recently-published volume of "Essays," advocates the view that the old idea of an essential difference, at all events of an opposition, between male and female elements, between so-called "sperm-cells" and "germ-cells," must be abandoned; that they are essentially alike, and differ only so far as one individual differs from another individual of the same species; and that fertilisation is no process of rejuvenescence, but merely a union of the hereditary tendencies of two individuals. This view he supports from the results of experiments which he regards as demonstrating the remarkable fact that, even in animals as high in the scale as the Batrachia, the "sperm-nucleus" can be made to play the part of an "ovum-nucleus," and *vice versâ*; it being possible to produce a free-swimming larva in the formation of which no "germ-nucleus" has taken any part; but, as far as the vegetable kingdom is concerned, the weight of evidence does not appear to be in favour of Weismann's hypothesis.

As has already been pointed out, very early in the evolution of the lower forms of vegetable life the motile zoospores began to assume sexual functions, and to become converted into zoogametes, and these again into active male and passive female elements; and, although the plasticity which is characteristic of the lower forms of life renders this differentiation for a time unstable, it becomes gradually more and more firmly established. Even in that class of beautiful Algæ, the Conjugatæ, of which *Spirogyra* and *Zygnema* are examples, where the "conjugating" cells show scarcely any difference even under high magnifying powers, it has now been shown that there is an essential difference between the male and female cells in their physiological constitution, and in their behaviour towards one another. Still more decisive is the fact, established by Auerbach in the animal, and by Rosen in the vegetable kingdom, that the male and the female nuclei behave differently to staining reagents, the former belonging to the class of "cyanophilous," the latter to the class of "erythrophilous" substances; that is, they take up especially blue and red colouring substances respectively. Other histological differences have also been pointed out between the male and female nuclei,

such as the possession of nucleoles by the former and not by the latter.

The act of fertilisation or fecundation appears to consist in the actual fusion of the nuclei of the male and female cells; at least this is the view taken by one of the highest authorities on this subject—M. Guignard—from a very careful series of observations on *Lilium martagon* and *Fritillaria imperialis*, in opposition to the theory of Van Beneden and some other observers. This fusion is assisted by the action of remarkable bodies which have only recently come under observation, and which have been termed "tinoleucites" or "directing spheres." These bodies, which have been observed by Guignard and Van Tieghem in plants, and by Fol and others in animals, are minute spherical masses of protoplasm occurring in both the male and female cells, which appear to exercise the function of directing the course of the male nucleus so as to bring it into contact with the female element. There appears to be also some directing principle in the higher Cryptogams which governs the antherozoids in finding their way to the oosphere within the archegone.

Without dogmatising on so intricate a question, the theory may at least be hazarded that every act of fertilisation is simply a modification of a process of nutrition, the conveying to a potential germ of some property which it does not derive from its own ancestors, but which gives it greater completeness, and endows it with greater resources in the struggle for existence. Sufficient, at all events, has been said to show how rich a field there is still open to the skilled microscopist in the investigation of the interesting problems connected with the perpetuation of animal and vegetable life on the surface of the globe. While it is probable that no human researches will ever lead to a solution of the questions: "What is life?" and "How were the phenomena of life first infused into inorganic matter?", we seem, year by year, to be able more closely to follow the steps by which, from the first germs of life, has been gradually evolved that marvellous complexity in the animal and vegetable worlds, the laws of which excite our increasing wonder and admiration the more minutely we investigate them.

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ALFRED W. BENNETT.

VI.

Animal Temperature.

THE higher animals have within their bodies some source of heat and some mechanism to regulate the production and loss of that heat, for, equally in the height of summer as in the depth of winter, their mean temperature is constant. This fact was known to the Ancients, but imperfectly; they had no thermometers, no exact methods of determining temperature. They judged from their sensations, but here sensations are imperfect guides.

A patient attacked by an ague feels chilly, miserably cold; he huddles up by a fire, he shivers, and his teeth chatter; he says that he is cold, but at this precise moment he is in a fever-heat. How does this contradiction arise? The feeling of heat or cold arises in the nervous structures of the skin. When these nerve-endings are flushed by a rapid stream of blood, there is a sensation of warmth; should the blood supply be small, not enough to compensate for the loss of heat by radiation and conduction, there arises a sensation of cold. During the *rigor*, for such is the name given to the early stage of ague, the vessels of the skin are contracted, the skin is pale and cold, its temperature twelve or sixteen degrees below the normal blood-heat; but, at the same time, the temperature of the internal parts has risen six or seven degrees above the normal.

Let the converse condition be studied in the same disease during its third or *sweating* stage. The skin is now red and flushed, its blood-vessels are dilated; the patient is bathed in perspiration; he says that he is intensely hot, although his internal temperature is rapidly falling to the normal.

Animal heat the Ancients considered to be beyond the reach of physical or chemical laws. They could assign no cause for it, and therefore looked upon it as some innate quality, something essentially "vital." This "vital" heat was supposed to be concentrated in the heart, and to be distributed to the body by the blood in the veins; it was prevented from accumulating by respiration, the chief function of which was to cool or temper the blood.

After the year 1595 it was first possible to determine the temperature of the body more exactly, and to make observations upon the independence of the temperature on external conditions. It was about this time that Galileo invented the thermometer. As observa-

tions now increased, numerous speculations as to the source of animal heat arose. It must be connected with the movement of the blood, for how otherwise could be explained the cooling of a corpse or of a limb deprived of its circulation? The heat might arise in the blood alone or have its origin in the heart, and only be distributed by the blood stream.

It was well known that heat arose during fermentation and by the contact of acid and base; animal heat was, therefore, considered to arise by some similar process or processes taking place in the blood. The former opinion was held by Willis, the anatomist, who, in his treatise "de Ascensione Sanguinis," written about the year 1670, gives the theory that there is in the blood a combustion which depends upon the fermentation excited by the combination of different chemical substances.

Friction was another well-known source of heat, and was the explanation given for animal heat by the celebrated Dutch physiologist, Boerhaave, who lived in the early part of the eighteenth century. He explained the "vital" heat as due to the friction of the particles of blood in the vessels.

A much more correct opinion had already been formed by Magin in 1674. This physiologist, after his experiments in the constitution of air and its relation to the heat of combustion, extended the analogy of combustion to animal heat. He held that the function of the lungs was not to cool the blood, but to enable that fluid to absorb the nitro-aërial spirit (oxygen) of the air, and so generate heat.

Black, in 1757, discovered that carbonic acid gas, which is formed by the burning of fuel, is also given off by the lungs; he thus showed a strict connection between the processes of combustion and respiration; he supported the theory that the heat arising from the union of oxygen and carbon was the real "vital" heat.

A few years later Lavoisier introduced a theory exactly similar to Black's, but how far it was dependent on or independent of Black's work is not known.

A serious objection was quickly raised to Black and Lavoisier's theory; it was shown that if the heat were produced in the lungs, the temperature of that organ would be incompatible with its life. To remove this objection, Crawford in 1781 proposed his ingenious theory, which he had based on experiments upon the specific heats of venous and arterial blood. His experiments appeared to show that the specific heat of arterial was considerably greater than that of venous blood; the combination, therefore, of the blood with the oxygen of the air in the lungs gave rise to the liberation of only a small amount of heat in the lungs; the blood here was arterial. When, however, the blood reached the systemic capillaries, it became venous, and the latent heat was rendered sensible. This was a very "materialistic" explanation of "vital" heat, and was, in certain respects, a distinct advance towards truth.

It is interesting that Crawford and Lavoisier were the first to make experiments on the amount of heat given off by animals, and even to compare the oxidation, as represented chiefly by the carbonic acid, with the amount of heat produced. They showed that the oxidation could produce the necessary heat.

John Davy in 1815 showed by more exact experiments that there was no perceptible difference in the specific heat of arterial and of venous blood, and therefore Crawford's theory must fall to the ground. This, however, by no means affected the doctrine of Lavoisier, that the oxidation in the animal body was the cause of the heat. In fact, Liebig not many years later, by properly interpreting the calorimetric experiments of Daling and Despretz, placed Lavoisier's doctrine on such a firm basis that it became strong evidence for the Law of the Conservation of Energy.

In the last fifty years the labours of Helmholtz, Ludwig and Pflüger have proved that the heat is formed in the tissues, that heat is produced by the activity of muscles and glands. Bernard also, by showing how variations of the circulation of the bood could be effected by the nervous system, threw a great light upon the process of heat regulation. The formation of heat varied in different parts, but the temperature was equalised and regulated by the continual blood-stream; the circulation of the blood, being under the control of the nervous system, could, when necessary, send more blood to the surface, and so increase the loss of heat, at another time could husband the warmth by diminishing the blood-supply to the skin.

Such have been the chief lines along which the knowledge of the causes of animal temperature has advanced. The doctrine of "vital heat," propounded by the vitalistic school, has fallen before the advance of materialism; "vital heat" is shown to be similar to heat arising from physical and chemical processes.

Numerous observations upon animal temperature have now accumulated. Of these the most interesting, without doubt, lies in the difference which has been found to exist between the lower and the higher animals. Those animals which are high in the scale of evolution, such as birds and mammals, have a high temperature, and this has a mean value (about 37° C. or 98° F. in man) which is independent of the temperature of the surrounding air. The lower animals on the contrary, such as molluscs, fishes, amphibians and reptiles, have a temperature generally only slightly above that of their surroundings. This difference between the two classes is expressed by the terms "warm-blooded" and "cold-blooded" animals.

The warm-blooded animal has in correspondence with its high temperature a very energetic oxidation within its tissues, and this must, *cæteris paribus*, be more vigorous in the winter than in the summer. The cold-blooded animals, on the contrary, have a much smaller oxidation, and one which is so lessened by cold that in winter

these animals find their energy so reduced that they pass the time in sleep—they hibernate.

The higher animals have been classed as the warm-blooded, the lower as the cold-blooded. This classification is, however, not absolutely exact. Indeed mammals are found, such as the hedgehog, bear, and dormouse, which are in an intermediate position, since, in spite of their high temperature in warm weather, their temperature falls in winter, they become inactive and hibernate, the oxidation of their tissues is reduced to a minimum; on the other side, there are bees, animals of a low order, which have and maintain a higher temperature than most cold-blooded animals, and are not reduced to spend the winter in slumber.

In order that an animal may have under very varying conditions a constant temperature, it must possess some regulating mechanism. There are two means by which regulation can be obtained, variation in the amount of heat produced and variation in the loss of heat. Both methods are found in use. When a white mouse, for example, is removed from cold to warm surroundings, it regulates its temperature by increasing the loss of heat from its body, it sweats and exposes to the air as large a surface of its flushed skin as possible; at the same time, its heat-production is lessened, it is less active, it gives off less carbonic acid, there is less oxidation in its tissues. Let the same animal be now returned to a cold chamber. At once its ears and face become pale, showing that the vessels of the skin contract, and prevent loss of heat by the blood passing to the skin; when not in active movement the animal huddles up together, ruffles its fur, and makes it as bad a conductor of heat as possible; it also regulates the heat production, as shown by involuntary shivers or active voluntary movements, it gives off more carbonic acid, there is increased oxidation in its tissues.

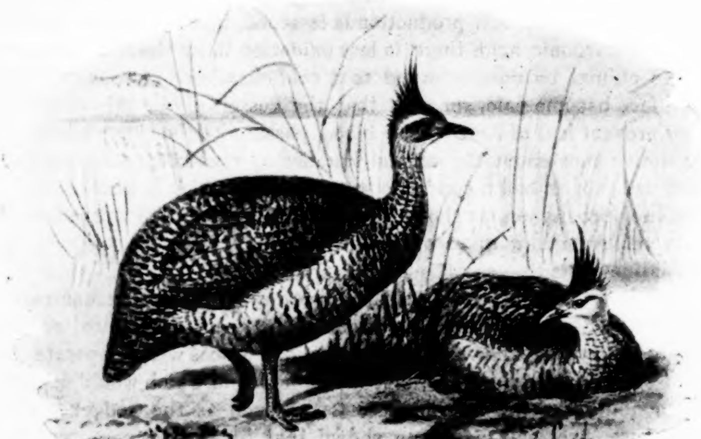
These regulating mechanisms appear to be under the control of certain parts of the nervous system. The exact anatomical seat is unknown, nor are known the anatomical differences which separate the cold-blooded from the warm-blooded animal, and which would explain the condition of the intermediate forms, such as the hedgehog, the dormouse, bees. It has been shown that by the action of certain nervous poisons, and by intense cold, warm-blooded animals may be made to pass into a condition somewhat resembling that of the cold-blooded animals. Ignorance now reigns here, but not so absolutely for ever.

M. S. Pembrey.

SOME NEW BOOKS.

Idle Days in Patagonia. By W. H. Hudson. 8vo. Pp. viii. and 256. Illustrated. London: Chapman & Hall, 1893. Price 14s.

If every sojourner in remote regions of the world occupied his "idle days" as profitably as has Mr. Hudson, our information regarding the animal life and its inanimate surroundings of the whole globe would be far less incomplete than it unfortunately is at present. We are, indeed, glad to welcome another volume by this author, written in the same charming and lucid style as his "Naturalist in La Plata," and embellished by illustrations executed in the same beautiful style. The present work is, however, far less exclusively devoted to the Natural History of the country it describes



Crested Tinamou *(Calodromas.)*

than was its predecessor, but for a vivid description of the mode of life in Patagonia, and the nature of the country, it can scarcely be surpassed. As with "La Plata," most of the chapters in the work before us have already seen the light in the form of articles in magazines and other serials, and this reproduction has given the author the opportunity of revising and amplifying his descriptions and maturing his opinions.

Confining our attention to those parts of the book devoted to the Natural History of Patagonia, we note that the author gives us excellent illustrations, accompanied with observations on their

mode of life, of the Black Vulture, the Upland Goose, and the Crested Tinamou, as well as of several of the smaller birds. Perhaps the most generally interesting chapter in the whole book is that on "Bird-music," in the course of which the author points out how impossible it is to convey any adequate idea of the songs of birds by mere description. He illustrates this by mentioning that before he had ever visited England he was anxious to obtain some idea of the nature of the melody of the British songsters, but that all his efforts were in vain. And the description of his delight at first hearing the music of our English fields and groves ought to remind us all how little we really appreciate, as a rule, the natural charms of our own land. Possibly some further development of the phonograph may be the ultimate means by which "Bird-music" may be rendered comprehensible to those who cannot hear it at first hand, as we greatly doubt if this can ever be effected by any merely graphic method.

Of scarcely less interest is the author's description of the ways of the leaf-bearing ants, on pp. 140–141; while those whose special study is Mammals will not fail to notice the observations on the burrowing Tuco-tuco (*Ctenomys*) and the Mara (*Dolichotis*). As the author well remarks, it is very curious to find in a burrowing animal the eyes of so large a size as they are in the former of these two rodents.

In commending this book to the attention of our readers, we may express a hope that it will ere long be followed by others from the same ready pen. R. L.

THE GREAT SEA-SERPENT: An Historical and Critical Treatise. With the reports of 187 appearances (including those of the appendix), the suppositions and suggestions of scientific and non-scientific persons, and the Author's conclusions. With 82 illustrations. By A. C. Oudemans, Jzn. Published by the Author, October, 1892. London: Luyac & Co.

Of this work we give two reviews, the one by a naturalist, the other by a literary contributor.

I.

THE author in his preface compares his work with that of Chladni on meteoric stones. Chladni, he says, opened the eyes of unbelievers by collecting and comparing all accounts of meteoric stones up to the nineteenth century. Meteoric stones were again found and were proved to be quite different from terrestrial stones. Unfortunately for the argument, remarkably few sea-serpents have been caught, and those few have proved to be not at all different from well-known objects.

We confess to have found Oudemans' book exceedingly dul reading. 379 pages are devoted to genuine or invented accounts of various appearances; 110 to explanations hitherto given. No doubt it is useful to have the literature of the subject compiled, but the author might have contented himself with a much greater compression of the interminable newspaper discussions, evidence on oath of sailors and fishermen, and so forth. We do not see that he advances at all beyond Mr. Hoyle's bright and short account of the sea-serpent in the "Encyclopædia Britannica." A number of the records are pure myths; some others are due to mistaken observations of floating sea-weed; porpoises swimming in a row; basking sharks, and so forth. The persistent records from the Norwegian coasts are most probably explained by the existence of gigantic cuttle-fish; and there remains

a residuum which quite possibly may be explained by the discovery of some new animal. Oudemans' own theory is given with a considerable amount of confidence, in some 50 pages towards the end of the book, and on page 516 an ideal sketch of the animal is produced. It is a Pinnipede to be called *Megophias megophias* (Raf.), Oud.; and a phylogenetic table of its relations with other Pinnipedes, living and extinct, is given. It is very possible that a large Pinnipede may exist, but, on carefully going through the characters suggested for it by this author, it is difficult to see that he has been guided in his selection from reports by any sounder principle than relying on what appeared to suit his hypothesis, and rejecting or explaining away inconvenient ones. Further notice of this book I leave to a literary contributor.

P. C. M.

II.

The attitude of the nineteenth century, social as scientific, towards the unknown may be summed up by the remark made by a lady member of the upper ten concerning those less happily situated in the social scale—"I don't know them: they don't exist." In the face of this attitude Dr. Oudemans has presented the public with a treatise of no less than 592 pages on our old friend the sea-serpent. We are warned on the title-page that the little volume contains reports of 187 appearances, "including those of the appendix," but we are not told what the appendix of the serpent is, and there are no special reports on appearances of his tail. The author much deplores the fact that his illustrations are not due to the unlying kodac. Nevertheless, the reader does not lose by this little omission in the baggage of observant travellers. It has been the means of providing portraits of the sea-serpent such as we feel assured no camera yet invented could have produced. The most impressive of these is one compiled by Messrs. Renard, *père et fils*, who combinedly took observations one moonlight night during a voyage on the high seas in 1881. The result is the portrayal of a dragon worthy the sublimest efforts of Sir Augustus Harris. We fancy even our own patron saint would have quailed before it, unless, indeed, following the advice given by Dr. Oudemans, he had armed himself with "explosive balls and harpoons loaden with nitro-glycerine." This particular specimen possessed not only teeth, "sharp, enormous, and white," but a phosphorescent tongue and an eye that looked backwards. Probably that eye also winked, for Dr. Oudemans includes both description and drawing under the heading of "Cheats and Hoaxes"—a section sternly dealt with at the beginning of the book. However, in the next chapter, entitled "Would-be Sea-Serpents," the illustrations are not to be outdone, in spirit and conception, by mere cheats and hoaxes, and in fig. 12, *Lineus longissimus*, Sow., we find something really worth looking at. Chapter iv. consists of reports and papers on the appearances of sea-serpents in various parts of the world. These are mostly compiled by naval officers, and a few—a very few—scientists. The Church, however, is not unrepresented, and we would not like to call in question the evidence of the Archbishop of Upsala, writing in 1555; but surely the Archbishop's illustration is the gem of the collection. The sea-serpent is depicted in the act of swallowing a sailor, and we can only charitably suggest that the original sketch was intended as a representation of Jonah's unfortunate experience. Among so much documentary evidence, it is difficult to discriminate critically. Even Milton is pressed into the service, quotations being given in "Reports

and Papers" from *Paradise Lost*, where the great leviathan is mentioned as being the special dread of the Norwegians (the coast of Norway being the favourite haunt of the monster according to the most reliable eye-witnesses). But why is Coleridge left out?

"Beyond the shadow of the ship,
I watched the water snakes.
They moved in tracks of shining white,
And when they reared the elfish light
Fell off in hoary flakes."

Later on, we come upon another picture of a sailor being cut off in his prime by the wily monster, and we pass on with considerable relief to Dr. Oudemans' "Conclusions," where, under the headings of "Harmlessness, Timidity, and Playfulness," we learn that the great sea-serpent is, after all, a mild creature, a conviction probably deduced from the fact that, if the great sea snake has ever shown his animosity to the unwary invaders of his haunts, no victim has, so far, survived to tell the tale.

It is sad, however, to find that Dr. Oudemans' great sea-serpent is, after all, only a monster sea-lion, and we don't know of his having done anything to justify Dr. Oudemans' calling him *Megophias megophias* (Raf.), Oud.

L. B. P.

HANDBUCH DER PALÆONTOLOGIE.—Palæozoologie, vol. iv., pt. i. Edited by K. A. Zittel. 8vo. Pp. 304. Illustrated. Munich and Leipsic: R. Oldenbourg, 1892.

WE are glad to congratulate Professor Von Zittel on the untiring energy which has enabled him to get well into the Mammals in the present fasciculus of this magnificent and invaluable work. It need hardly be mentioned that the whole of the Vertebrate portion of the work has been written by the Editor himself; and the present fasciculus is fully equal to its predecessors in the care and attention

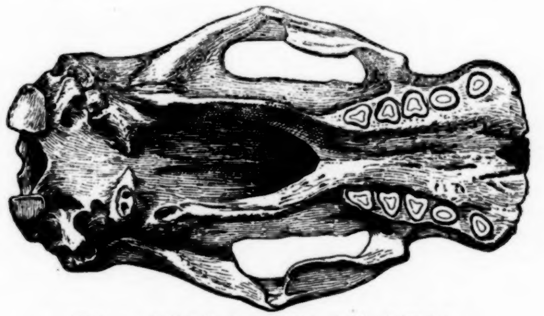


FIG. 1.—Palatal view of the skull of *Mylodon*.

which has been bestowed upon it, and in the excellence of the illustrations. Three of the latter we are enabled to reproduce as samples. We are especially glad to notice that the author has devoted particular attention to the recent important discoveries which have been made among the fossil mammals of South America, and that a host of new forms have been assigned to their proper serial positions. This is the more creditable to the author, seeing that owing to unfortunate circumstances the vertebrate palæontology of that country

has been involved in an intricate labyrinth of confusion, from which it can only with much labour and care be freed. We are persuaded, however, that in accepting in the main the conclusions of Señor Ameghino, the author has been well advised ; and we are glad to see the newly-discovered Argentine Tertiary Marsupials placed next the living Australian Thylacine.

Almost the only portion of the work that we do not like is the classification which the author has seen fit to adopt. In place of dividing Mammals into the three primary groups of Ornithodelphians (Monotremes), Didelphians (Marsupials), and Placentals, Professor Zittel takes only two primary groups, namely, Eplacentals and Placentals ; the former including the Monotremes and Marsupials, together with the extinct Allotheria, or Multituberculata. Now, we are fully prepared to admit that there is much to be said in favour of a binary instead of a ternary subdivision of the Mammalian class. If, however, such binary scheme of classification be thought advisable, we have no hesitation in saying that the Monotremes (together with the Multituberculata) should form one subclass, while the Marsupials should be brigaded with the Placentals in the second, and that there is no sort of justification for the scheme followed by our author.

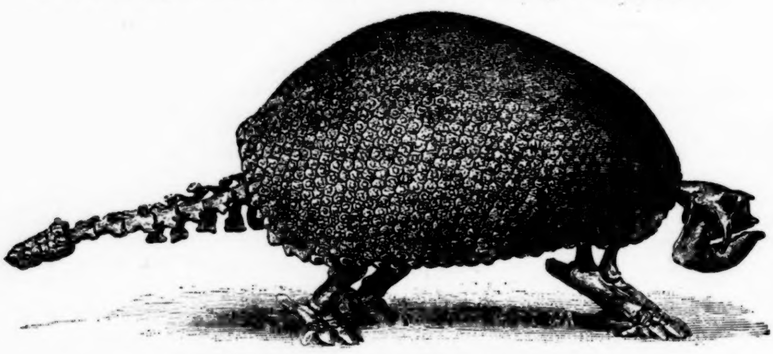


FIG. 2.—Restored skeleton of *Glyptodon*, with the tail incomplete.

The present fasciculus includes the Monotremes, Multituberculata, Marsupials, Edentates, Cetaceans, Sirenians, and the Condylarthrous and Perissodactylate sections of the Ungulates.

A large space is devoted to the Mesozoic Mammals, among which the Allotheria (Multituberculata) are assigned a rank equivalent to that of the Monotremes, while all the other forms are included (and we believe rightly) among the Marsupials, some of the Cretaceous types being even placed in an existing family. There may be some justification for making the Banded Anteater (*Myrmecobius*) the representative of a distinct family, but there is surely none for placing the Peramelidæ between that family and the Dasyuridæ. Then, again, we must take exception to making the Rat-kangaroos a distinct family (to which, by the way, a wrong name is given) separated by *Thylacoleo* and the Phalangers from the Kangaroos (Macropodidæ).

In the Edentates, special interest attaches to the excellent restorations of the skeletons of the Glyptodonts, one of which is herewith given. We are, however, persuaded that the division of the Ground-Sloths into several distinct families is not justifiable. We notice that the author has seen no reason to follow certain new views

which are supposed to justify the removal of the extinct Zeuglodonts from the Whales.

Beyond the circumstance that a large number of the newly-discovered South American forms are described and figured, the only point in the part devoted to the Ungulates to which we need direct attention is in regard to the classification of the Perissodactyles. Following recent American views, the author adopts a phylogenetic classification, and includes in the same family such widely-different forms as the modern *Equus* and the Eocene *Hyracotherium*. On the other hand, *Systemodon*, which is very close to the latter, is placed in the Tapiridæ. That these two Eocene forms may be the ancestral types of the two families to which they are respectively referred, we are not going to dispute for a moment. We do say, however, that to include them in such families, and to separate them from one another, is, to our mind, only confusing classification. Moreover, if we are to have a phylogenetic classification, the author does not go far enough.

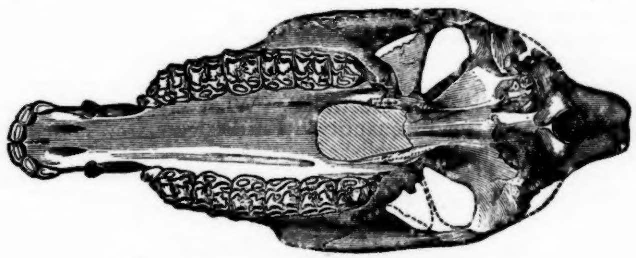


FIG. 3.—Palatal aspect of the skull of *Hipparion*.

Surely, seeing that the Condylarthra are admittedly the ancestors of the Perissodactyla and Artiodactyla, there is just the same justification for putting all three in a single group, as there is for refusing to admit that there may be an Eocene family (Lophiodontidæ) which shall include the ancestors of both the modern Equidæ and the Tapiridæ. As an example of the illustrations among the Equidæ, we reproduce one of the skulls of *Hipparion*.

That the work (however much we may be disposed to differ from it as regards points of detail) when complete will be of inestimable value to the palæontologist and zoologist, we have already had occasion to say elsewhere, and our only regret is that the limited cult of the former science in this country forbids the publication of one on the same lines in England. R. L.

ÉLÉMENTS D'ANATOMIE COMPARÉE. By Remy Perrier. Pp. 1,000. With Illustrations. Paris: J. B. Baillière et Fils, 1893. Price 20 francs.

ZOOLOGICAL text-books seem at the present time to be multiplying at a rate altogether out of proportion to the numbers of the students for whom they are intended. This is not only the case in this country, but also on the Continent. The commencing student of zoology has so many works to choose from (we cannot, however, justly say that there is an *embarras de richesses*) that he probably ends by taking one at random. It would be much better if the elementary student would refrain altogether from purchasing text-books, and trust himself to the

professor whose lectures he attends. However, if he must buy a text-book, then let him consider two standards by which, in our opinion, a book of this kind should be judged. A book intended for elementary students ought to be of moderate bulk, and it should be thoroughly up to date. In these days of examination, the very newest discoveries are apt to be inquired about by examiners. The book should be rendered fairly portable through judicious condensation, not so pronounced, however, as to produce obscurity. The writer of a text-book should remember that human flesh is weak, and also that time is short. M. Perrier's book is not, judged by these standards, altogether ideal; still, we gladly admit that it has its good points. Possibly, too, the French student is differently placed with regard to examinations. The author might, however, if he had taken thought, have refrained from adding an inch or two to the stature of the book; this would have been a distinct gain, for we have seldom handled so awkwardly shaped a volume. If there is a second edition it is to be hoped that a fission into two equally-sized halves will take place.

A feature of M. Perrier's book is the introduction of coloured plates, illustrating, of course, the circulatory system of various animals. The illustrations are not generally very first-rate, and the same remarks apply to the woodcuts and "process" blocks by which the text is really very lavishly illustrated. What is wanting in quality is certainly made up in quantity. Furthermore, the author pays more attention to papers published outside France than is sometimes the case with his countrymen. Altogether, we are favourably impressed with M. Perrier's book.

TEXT-BOOK OF THE EMBRYOLOGY OF MAN AND MAMMALS. By Dr. Oscar Hertwig. Translated from the third German edition by Edward L. Mark, Ph.D., Hersey Professor of Anatomy in Harvard University. Pp. 670, with 339 figures in the text and 2 lithographic plates. London: Swan Sonnenschein & Co., 1892.

ALMOST as much as in the case of Balfour's great treatise on comparative embryology, this book is the result of the author's original investigation. The two books have very different purposes. Balfour was practically inventing a new science, and he endeavoured, so far as could be done, to go over the whole animal kingdom, and set forth, in orderly and systematic exposition, the facts and the principles of Comparative Embryology. The very result of the enormous stimulus to research given by his treatise is, that the treatise is now so largely behind our knowledge that it is rather a landmark in history than a text-book for beginners. Dr. Oscar Hertwig has written his book specially from the point of view of the needs of those studying human anatomy. The facts and principles of comparative embryology are used chiefly as a guide to the ontogeny of higher forms. But the student of medicine or morphology who masters this volume will have a large knowledge of comparative embryology, and will have the unity of the animal kingdom strongly impressed on him. It would be difficult to think of anyone from whom a book of this kind would be more valuable, for a large part of the extension of our knowledge has been due to the studies of Oscar Hertwig and his brother on the Cell and the Coelom-Theory.

Those who have been unable to follow recent developments of embryology closely, will be most interested by the chapters on the middle layer. Not long ago it was assumed that the mesoblast was a

germinal layer of equal value with epiblast and hypoblast. True, it appeared later in development, and its mode of origin was in dispute. Here there will be found a different conception. Hertwig divides Metazoa into animals with two germ layers and animals with four germ layers. In the latter, the two middle layers are both derived as definite organs from the inner layer; the one is the median notochord; the other, the paired sacs of the body cavity. In Amphioxus alone, this condition is perfectly definite; but the recent discovery[1] of segmental ducts, and nephridia in Amphioxus certainly will increase the content of morphologists in regarding Amphioxus as a representative of the ancestral Chordata. In higher forms, the presence, actual or ancestral, of masses of food-yolk has so compressed the pouch-like evaginations from the primitive gut that they arise as solid outgrowths.

Hertwig keeps by themselves these four primitive layers as "Epithelia," which serve for the limitation of the surfaces of the body. At the close of segmentation only one layer is present—the epithelium of the blastula. From this the other layers arise by infoldings and outfoldings. But there is another element of development. In the Cœlenterates and Echinoderms, when the two primary layers are established, there is found between them a supporting gelatinous layer. There wander into this from the primary layers cells most often starting from the point where these layers pass into each other. These immigrant cells lose their epithelial character and spread out as branched cells like connective tissue corpuscles. This factor in development is called by Hertwig the "mesenchyme," and he finds it in the development of all higher forms as the origin of the connective tissues and the blood.

Agreeably to the intention of the book as a text-book, this theory is laid down crisply and definitely, and it certainly is an extremely important advance on current ideas, although, for reasons that will readily occur to specialists, it may well be that the two middle layers and the mesenchyme do not exhaust that complex telescoping of forgotten stages in the development of organs we call mesoblast.

For the printing, illustrations, index, and references to literature, the highest praise is due. The translator has done his work well, but he is not to be congratulated on his translation of the word "anlage" —a translation on which he plumes himself so much in the preface. "Fundament" is a clumsy word, and both the noun and adjective have meanings already in possession. As an English equivalent, "forecast" is the best word, and for an adjective "incipient" is very convenient.

P. C. M.

Die Zelle und die Gewebe. [The Cell and Tissue.] By Dr. Oscar Hertwig. Pp. 300, with illustrations. Jena: Gustav Fischer, 1892. Price 8 marks.

A book treating of the structure of cells, written by Dr. Hertwig, and published by Herr Fischer, of Jena, is sure to be excellent in matter and in style. The volume before us is, on account of its very excellence, difficult to use as a text for a short discourse in this review. Merely to praise is apt to produce rather laborious reading. The only proper alternative would be to give a general account of the book; but this would occupy a little too much room; we shall therefore

[1] Since the publication of this edition.

content ourselves with noting a few points. The subject of which Professor Hertwig treats is one which the Germans have made particularly their own; it is true that eminent persons, not Germans, such as Professor Edouard van Beneden, have investigated these matters, but the bulk of the work has been done in Germany.

Professor Hertwig is, of course, a zoologist, but he has by no means neglected the botanical side of his subject; indeed, the work would be comparatively valueless if he had. It is one of the most remarkable generalisations of Biological science that animal and vegetable cells are identical in all important characters. This was first realised when the identity of their protoplasm was proved by Von Mohl; and the discovery of the nucleus, first due to our countryman, Robert Brown, led the way towards proving another fundamental resemblance between the structural units of the plant and the animal. Finally, the recent discoveries that the phenomena of the dividing nucleus termed karyokinesis were not peculiar to the animal cell, but also characterised the vegetable cell, left nothing wanting to prove the close similarity of the two. There are still remaining certain cells in which a nucleus has not been discovered; on the other hand, the balance is restored by the existence of other cells which have more than a fair share of nuclei. As to the former class, it is doubtful whether a nucleus is ever really wanting. The mammalian red blood corpuscle will at once occur as an instance of such a cell; but, as Professor Hertwig points out, these bodies are not the equivalents of cells at all. Some of the simpler amœboid organisms appear to be without a nucleus, and it was suggested that in such cases the organism was really a free nucleus, with little or no protoplasm. This suggestion was made, we believe, by Dr. Will; Dr. Hertwig alludes to the matter, but does not quote Dr. Will. The Bacteria are organisms whose real nature has been disputed; some have been unwilling to allow to these "plants" the rank of a cell. Bütschli, however, found deeply staining bodies in Bacteria to which he assigned the value of a nucleus. Dr. Hertwig does not refer in this connection —as he might have done—to Mr. Harold Wager's investigations into the nuclei of Bacteria, communicated to the British Association at Cardiff.

An important part of this work deals with karyokinesis. To the general account is appended a brief historical sketch of the matter, and a discussion of the more debatable points, and among these the origin of the centrosoma is the principal one. After giving the arguments in favour of its nuclear derivation, the author ends with the opinion that the time is not ripe for a definite settlement of the question. In the current number of the *Quarterly Journal of Microscopical Science* is a valuable paper upon these matters.

An interesting section of the book deals with the position of the nucleus in the cell—that is, its relation to growth, deposition of "formed substances," &c. The nucleus is all-important; a cell, in fact, is a territory governed by a despot, the nucleus presiding over all the activities of the protoplasm. It was pointed out a few years ago that the yolk in the ovum was first formed in the neighbourhood of the nucleus, a discovery that entirely does away with a view at one time current that the yolk, or, at any rate, a good deal of it, was not of home manufacture, but fabricated outside the ovum, and conveyed to it through the pores in the egg-membrane. Dr. Hertwig quotes and illustrates from the observations of Haberlandt upon the relation of the nucleus to thickenings in the cell-wall of vegetable tissues, and to the growth in length of cells; when these circumstances are

occurring, the nucleus is always apparently to be found at the seat of action. Finally, there are the important studies of Gruber and others upon the artificial breaking-up of cells; in such experiments it has been found that a fragment of the original cell in which the nucleus is left can be regenerated anew, while the remaining pieces live for a time, but eventually decay. The book is, in fact, a very complete and impartial summary of existing knowledge upon one of the most important and fascinating branches of Biology; but it is, of course, written by a specialist for people who are acquainted with, at least, the outline of the subject. Nevertheless, in the proper sense of the word, it ought unquestionably to be a most popular work.

THE GAME BIRDS AND WILD FOWL OF THE BRITISH ISLANDS: Being a Handbook for the Naturalist and Sportsman. By C. Dixon. 8vo. Pp. xvi. and 468. Illustrated. London: Chapman & Hall, 1893. Price 18s.

THE stream of books on British Birds seems to be an endless one, but, nevertheless, there is doubtless an opening for the present volume, which deals with all those groups of special interest to the sportsman, namely, the game birds properly so-called, the pigeons, rails, plovers and their kin, as well as the water-fowl. In his preface, Mr. Dixon—whose name is already well-known through several popular works on ornithology—is careful to observe that he is largely indebted to the writings of others for much of the material on which his work is based, although his own ornithological studies have enabled him to add a considerable amount of original matter connected with the habits of the birds described. Since, we believe, there is no other work of this kind covering precisely the ground occupied by the volume before us, Mr. Dixon's handy and well-illustrated manual ought to be acceptable to all sportsmen who take interest in the natural history and habits of their quarry.

We cordially agree with the author's prefatory remarks concerning the subject of bird classification, and the haste with which almost every aspirant to ornithological knowledge propounds some startling new scheme, destined to revolutionise the science—until superseded by the next attempt; and we think he has done well in keeping more or less closely to one of the older arrangements. In attaching the termination "*formes*" to each one of the old ordinal names, such as "Anseres," we are of opinion, however, that he is ill-advised; the only possible advantage of such a cumbersome affix being in cases where the terms are employed in the sense of subclasses, as, for instance, when Mr. Sharpe uses the name "Anseriformes" to include the order "Anseres" and several others of the old ordinal groups. In placing the Sand-Grouse under the "Galliformes," rather than with the "Columbiformes," the author has no sort of justification, as the osteology of these birds is alone amply sufficient to show that their affinity is much closer with the former than it is with the latter group. Neither are we yet convinced of the propriety of removing the Bustards from the vicinity of the Cranes, and placing them with the Plovers.

The mention of Cranes reminds us that under the heading of the Demoiselle Crane, the author states that the time during which this bird may be taken is "August 1st to March 1st, otherwise by authority of owner or occupier of land." Now, seeing that there is but one solitary record of the occurrence of this species in the British

Islands, it strikes us that the introduction of the statement in question is a trifle superfluous. Indeed, in a book intended primarily for the sportsman, we should have thought even any mention of this particular species of Crane might have been perfectly well omitted.

These are, however, but unimportant blemishes in a work which we can thoroughly recommend to the attention of the class of readers for whom it is specially designed, as being both readable and, so far as we can see, at the same time accurate and up to date.

R. L.

THE HEMIPTERA HETEROPTERA OF THE BRITISH ISLANDS. By Edward Saunders, F.L.S. 8vo. Pp. viii. and 350, with a structural plate. London: L. Reeve and Co., 1892. Price 14s. (Large paper edition with 31 coloured plates, price 48s.)

IT is with pleasure that we chronicle the appearance of this work, which is uniform with Canon Fowler's "Coleoptera of the British Islands," lately issued by the same publishers. Since the appearance of the Ray Society Monograph of the British Hemiptera by Messrs. Douglas and Scott in 1865, and Mr. Saunders' own excellent synopsis of the order in 1876, numerous species of bugs have been added to our fauna, and inevitable changes and re-changes in nomenclature have been made. The classification and synonymy of the British Hemiptera are now brought thoroughly up to date, and it is to be hoped that the book will incite many naturalists to the study of these interesting insects, which, though much neglected in comparison with moths and beetles, well repay the student for his work, and in some respects offer him more opportunity for discovery than the more popular groups.

Only 12 pages are devoted to an account of the affinities, anatomy, and development of the Hemiptera, and the methods of their capture and preservation. It is to be regretted that this portion of the work has not been carried to a greater length; collectors, even now, too often think that to give a specimen a name is the only end of their study; and attention might well be directed to some of the morphological problems towards whose solution the commonest bug may contribute material. Short though it is, however, this introductory portion is clear and reliable, and as far as external structure is concerned, well illustrated by the plate.

The book, however, is issued as a systematic work and, as such, it is excellent. In his arrangement the author follows Puton; entomologists familiar with the sequence of the families in Douglas and Scott will notice that the Hydrometridæ and Hebridæ are brought forward to the middle of the series between the Aradidæ and Reduviidæ, while the Capsidæ are relegated to the end. There are synoptical tables of the genera and species which must have cost the author great trouble, and which, in conjunction with his full and accurate descriptions, should safely guide the student to correct determinations. Those who can afford the large edition will find, in some cases, the coloured figures a "royal road" to identification; but among the more critical groups, such plates are of doubtful utility. The Hemiptera are classified in the main not by colour, but by structure; and outline figures of the distinctive parts in nearly allied genera and species would be of more value than the coloured plates, good as these are. In the table of the genera of the Capsidæ,

for example, which extends over six pages, and in which the differences are often relative, a few good outlines would be invaluable to the beginner. We could also wish to see figures of the immature stages of some of the Hemiptera.

The descriptions are occasionally relieved by accounts of habits, and a full list of the known British localities for each species is given. In a group so comparatively little worked, these localities are at present, probably, as much a guide to the distribution of hemipterists as to that of Hemiptera; the southern counties of England, and Norfolk, however, seem to have been so well searched that species absent from them may be presumed to have a northern or western range in the British Islands. It is a pity that Mr. Saunders has not given at least the outlines of the distribution of each species abroad. A knowledge of this must go hand in hand with that of the British range of an animal, to enable the student to form conclusions as to the relative age of allied forms, and the time and method of the introduction of the various elements of our fauna. Mr. Saunders, in his hints on collection and preservation, lays stress on the necessity for recording localities. It is to be hoped that collectors will all take his advice. Entomologists now, happily, recognise that the value of a specimen is vastly increased when its locality is known; the patient accumulation of such records may furnish invaluable material for future workers at animal distribution and its allied problems.

Altogether Mr. Saunders' work must be considered a most valuable addition to the literature of the British fauna.

THE FAUNA OF BRITISH INDIA, INCLUDING CEYLON AND BURMA: MOTHS. Vol. I. By G. F. Hampson. 8vo. Pp. xxiv. and 527, with 333 woodcuts. London: Taylor & Francis, 1892.

THIS is the first volume on invertebrate animals which has appeared in the excellent series of Indian faunistic works edited by Dr. W. T. Blanford. It contains synopses of the families of moths, and of the Indian genera and species of most of the families included under the terms Sphinges and Bombyces. Mr. Hampson, however, rejects all tribal divisions of the Lepidoptera except the primary separation into butterflies (Rhopalocera) and moths (Heterocera), and he appears to consider the groups of Noctuids, Geometers, Pyralids, &c., as of only family, and not tribal, value.

After a short introduction on the external anatomy of the Lepidoptera, there is a "tree" showing the supposed relationships between the families of moths, followed by a table in which these families are differentiated, mostly by the neuration of the wings. Mr. Hampson has followed, in the main, the classification propounded by Snellen in his "Vlinders van Nederland." British lepidopterists will notice that here, as in Mr. Kirby's recent "Catalogue of Lepidoptera Heterocera," the Sphingidæ are deposed from their place at the head of the moths, and placed next the Notodontidæ. In Mr. Hampson's genetic tree the Saturniidæ appear at the head of one high branch, while the Epicopiidæ, Uraniidæ, and Geometridæ figure at the summit of the other. The Micropterygidæ and Hepialidæ are considered the lowest families, though removed from the main stem, on which the Tineidæ take the lowest place.

The descriptions of the genera and species are illustrated by cuts, the pattern of the wings being generally shown on one side, and their neuration on the other; details of the antennæ, palps, &c.,

are also often given. Mr. Hampson has "lumped" not a few "species"; for example, *Protoparce orientalis*, Butl., is given as a form of the European *P. convolvuli*, L. It is becoming more and more evident that in many groups of Lepidoptera the terms "species" and "variety" must be used according to the taste of the author.

We are promised two more volumes of the work during the next three years, bringing the series down to the end of the Pyralidæ. The remaining families are to be worked out by Lord Walsingham. The volume before us will be gratefully received by students of Indian moths, as it meets a real want, and will prove an excellent guide to future workers.

DOVER COAL BORING: Observations on the Correlation of the Franco-Belgian, Dover, and Somerset Coal-fields. By Francis Brady, C.E. June, 1892. With Reports by M. Victor Watteyne, Mons; James McMurtrie, F.G.S., Radstock; and M. R. Zeiller, Paris. [No place of publication, printer, or date; but Watteyne's Report is dated 16 July, 1892; McMurtrie's, 8 October, 1892; and the section, 19 December, 1892.] Pp. 41. Map (South Wales to Liége), and detailed section of boring.

THIS is the official report on the boring to the westward of the Shakespeare Tunnel, Dover, sunk at the instance of Sir Edward Watkin, on behalf of the Channel Tunnel Company, in search for workable coal seams thought to occur at this point. Although the various Reports differ somewhat, the mysteries surrounding the venture are now dispersed by the detailed section, from which we gather that the following beds were penetrated, commencing at the bottom of a 44 ft. shaft of 9 ft. diameter:—

Depth. Ft.		Thickness. Ft.	in.
91	Lower Grey Chalk	91	0
130	Indurated Chalk Marl	39	0
138	Chloritic Marl	8	0
186	Light-coloured Gault	48	0
256	Dark blue Gault	70	0
259	Sandy Gault	3	0
1113	Various beds representing the Lower Cretaceous and Jurassic	854	0
1136·6	Shale, Sandstone, and Bind	23	6
1140	Coal	2	6
1229	Shales, &c., (with a 6 in. Coal)	88	6
1231	Coal	2	0
1277	Shales, &c.	41	0
1279	Coal	2	0
1311·9	Shales, &c.	25	0
1313	Coal	1	3
1433	Shales, &c.	120	0
1434	Coal	1	0
1456	Shales, &c.	22	0
1458·6	Coal	2	6
1570	Shales, &c.	109	6
1572·3	Coal	2	3
1763·9	Shales, &c.	191	6
1766·6	Coal	2	9
1831	Shales, &c.	64	6
1832·8	Coal	1	8
2038	Shales, &c.	207	0
2039	Coal	1	0
2177·6	Shales, &c.	138	6
2181·6	Coal	4	0

In the report by Mons. Watteyne, which follows the section, we read: "The Coal-measures were reached in beds practically horizontal,

and it was bored in them as deep as 1,940 ft., the horizontal bedding being continuous. In that thickness of 783 ft. seven seams, being more than 1 ft. 6 ins. thick, were found. . . . Two facts are to be quoted about the Dover boring—the *horizontality* and the *regularity* of the beds." But this, it is pointed out, may be due to the boring being just made in the axis of a fold. From analyses made by M. Watteyne, he believes the coal to be of the same quality as the "gras flambant" of the Belgian basin. Under similar conditions of thickness and depth, the average cost of working the Hainaut seams in 1890 was 10 fr. 33 c. (8s. 3d.) per ton.

In Zeiller's report on the fossil plants (which has already appeared in the Comptes Rendus of the Academie des Sciences) the following lists are given:—1,894 ft.: *Mariopteris sphenopteroides* (?), Lesq.; *Neuropteris scheuchzeri*, Hoffm.; *N. rarinervis*, Bunb.; *N. tenuifolia*, Schl.; *Lepidodendron aculeatum*, Sternb.; a *Cordaicarpus* (?) *congruens*, or *Carpolithes corculum*, Sternb. 1,900 ft.: *N. scheuchzeri*; *N. rarinervis; N. tenuifolia; Cyclopteris; Calamophyllites goepperti*, Ett.; *Lepidostrobus variabilis*, L. and H., and *Cordaicarpus*. 2,038 ft.: *N. scheuchzeri; Lepid. lycopodioides*, Sternb.; *Stigmaria ficoides*, Sternb. M. Zeiller concludes his report with these words:—It may therefore be concluded from the presence of these two species [*N. rarinervis* and *N. scheuchzeri*] in the Dover boring, that, as presumed by Mr. Brady, the beds traversed by this boring rightly belong to the upper region of the Middle Coal-measures, and, if one may state it more precisely, that they cannot be either more recent than the beds of Radstock, in Somerset, or more ancient than the deepest beds of the upper zone (*charbons gras et fléxus*) of the Pas de Calais.

Analyses of the Dover Coal (lower seams) gives 83·80 carbon, 4·65 hydrogen, ·97 nitrogen, 3·23 oxygen, with heating power in units of 14,867. Eight workable seams have been met with (a total thickness of 16 ft. 11 in.) between 1,181 and 1,875 ft. from the surface, while the lowest yet met with, 4 ft. thick, and 2,222 ft. from the surface, is, as pointed out by M. Watteyne, well within payable depth, as worked in Belgium. The rest of this interesting pamphlet is taken up with commercial speculations, hopes, and anticipations.

Le Caoutchouc et la Gutta-Percha. By E. Chapel. Pp. 600. Paris: 1892. Price 20 francs.

This work, by the Secretary of the "Chambre Syndicale des Caoutchouc, Gutta-Percha, etc.," gives a complete history of the origin, composition, and manufacture of the industrial products mentioned. The first 80 pages of the book deal with the history of the substances, *i.e.*, the early discoveries and uses, vulcanisation, etc. The second part, pp. 81–295, treats of the botany and geographical distribution of the trees producing the exudation, and appears to collect together much that is valuable both from a botanical and an industrial point of view. Pages 297–340, deal with the physical properties, composition, action of heat and reagents, vulcanisation and its alterations, and the elimination of the sulphur, while the fourth part (pp. 341–519) is devoted to the manufacture and uses. The fifth division of the book is reserved for Gutta-Percha, which is treated in the same systematic way as Caoutchouc; and the sixth and last part deals exclusively with the commercial questions, export and import, custom dues, publications especially concerned with the industry,

and similar matters. There are many illustrations, and the book is interesting not only to the manufacturer but to the general reader.

A TEXT-BOOK OF TROPICAL AGRICULTURE. H. A. A. Nicholls, M.D., F.L.S. 8vo. Pp. xxi. and 312, with illustrations. London: Macmillan & Co., 1892. Price 6s.

THIS book is the outcome of a competition. The Jamaica Government offered a premium for the best Text-book of Tropical Agricuture specially adapted for the use of colleges and higher schools in that colony, and the author having sent in the manuscript of part of the book before us, it was awarded the prize on condition that some chapters were added treating of most of the cultivated tropical plants not noticed in the original. This was done, and in 1891 the Government of Jamaica published the book, of which Messrs. Macmillan have just issued the London edition, of the convenient size, general excellence of production, and with the bright scarlet cover of their other manuals for students.

The work has already passed the test of practical experience in Jamaica, and is now being adopted by the Governments of other colonies.

The author hopes to serve not only scholastic institutions, but peasant proprietors, owners of small estates, and intending settlers in tropical countries, and the fact, as stated in the preface, and evident from the merest glance at the text, that we have here "not a mere compilation, but the record of experience gained by study, observation, and experimental cultivations," should ensure it success.

The book is divided into two parts. Part I.—"Elements of Agriculture"—is a capital introduction to the science, occupying 85 pages, well arranged in thirteen chapters. The first is brief and introductory, the second and third give a concise account of Soils, how they are formed by atmospheric and other action, their constituents, classification, and properties. Chapters 4 and 5, on Plant Life, tell in about 20 pages of the parts of a plant, its nutrition, composition, and reproduction by seed or vegetative processes. A short chapter follows on Climate, "the greater or less degree of heat, light, and moisture," and the influence thereon of elevation, forest, aspect, or soil. Then under manures is considered the restoration of fertility to an exhausted soil, or the improvement of a poor one, and the most important "general" and "special" manures, their preparation, use, and effect, are described.

Chapter 8, "Rotation of Crops," follows, and is, unfortunately, necessarily short, very little attention, as the author remarks, having been given to working out a proper system of rotation in the West Indies, as has been done in Europe and North America. The importance of experiments is urged, and a course of rotation suggested, viz., yams or tanias (the rhizome of *Colocasia esculenta*) the first year, maize the second, sweet potatoes the third, and castor-oil or a similar crop for the fourth. Stress is also laid on the beneficial mechanical effect on the soil, from the necessary frequent turning over and consequent exposure to the chemical action of the atmosphere, while the rotting of roots of former crops make channels in all directions for water and air. Moreover, a proper system of rotation prevents blight and keeps away destructive insects

that confine their depredations to particular plants, as these pests will be starved out in the seasons intervening between the recurrence of their especial crop.

Drainage, Irrigation, and Tillage Operations form the subjects of the next three chapters, the third containing a description of the various instruments and their uses. Then a few words on Pruning, which, it must be remembered, is a scientific operation quite different from hacking or mere reduction of the bulk of a tree, and finally a chapter (13) on Budding and Grafting; this, by the way, with some good illustrations.

Part II.—Agriculture Products—occupies the remainder or rather more than two-thirds of the book. It is a most interesting account of the principal plant-products of our West Indian Colonies, and the mere general reader, as well as the man of science, will find it so, comprising as it does a well-written history of the growth and preparation of such everyday commodities as coffee, cocoa, tea, sugar, spices, drugs, dyes, or tobacco, " most universally used by mankind."

Tropical cereals, fruits, and food-plants like cassava, arrowroot, yams, and others, are also considered. Take, for instance, the chapter on Coffee. The idiosyncrasies of the two cultivated species, both of which hail from Africa, *Coffea arabica*, and *C. liberica* are mentioned, then its propagation in the seed-beds or nurseries is described, and a word is thrown in in favour of bamboo-pots, made by sawing the stem through an inch or two below each node, as strong and inexpensive. Then follow the preparation of the land, holing, planting out, the necessity for shade, weeding, topping, pruning, manuring; a "catch crop" may be reaped with advantage while the shrubs are growing; the vacant ground between the rows being planted with maize, plaintain, sweet potatoes, or other such food-products. A brief account is given of the enemies of the coffee trees and how to meet them; and, finally, the gathering of the crops and the preparation of the berry for market.

A similar plan is followed in the case of other products, and, in a few cases, sketches of the plant or shoot with the flower, fruit, or seed are also given. These sketches might have been more numerous and exhaustive; they are generally small and convey but little information.

Dr. Nicholls has, of course, dealt only with the West Indian products, some of which are, however, universal in the tropics, but the principles inculcated are so general, that the work will be valuable in the Old as well as the New World, and deserves to be widely known.

LE THÉ, botanique et culture, falsifications et richesse en Caféine des différentes espèces. By Antoine Biétrie. Small 8vo. Pp. 156. With 27 figures in the text. Paris: Baillière et Fils, 1892. Price 2 francs.

STRUCK by the ignorance of most of its consumers as to the cultivation and preparation of tea, by the consequences of its immoderate use, and the numerous methods of estimating the active element therein, the author of this little volume was induced to study some of its properties both from a botanical and chemical point of view. His account should especially interest English-speaking people for, from the statistics given on page 11, they are by far the greatest consumers; the Australian heads the list with 2·9 kilogrammes per person, then follow the English and Canadian with 2 and 1·98 respec-

tively, the French are next with only ·9; then the American with ·6; the Dane with ·2; and other European nations with a rapidly decreasing amount down to the Italian, who is presumably satisfied with ·001.

The book is divided into three parts; the first, dealing with "Botany and Culture," begins with a botanical description including a good figure of a shoot, and also sketches of the leaf and parts of the flower. Chapter II. tells of a few unproductive attempts at European cultivation, and gives a very brief account of the culture and the three yearly pickings in China. Then the preparation of green and black tea is described, the difference lying in the treatment of the leaves; the author expresses his belief in the view that the black owes its colour to the fermentation the leaves have undergone, to which is also due the much greater percentage of ammonia salts than in the green, the proportion being as 40 to 13. Anyhow, as we read in chapter IV., tea is a delicious beverage, though liable to be spoilt in the making; accordingly we are told how they make it in China *fide* General Tcheng-Ki-Tong. In Japan they do it a little differently, and in France differently again; the English method, which strikes us as infinitely superior to them all, is not given.

The physiological action includes not only the well-known effect on the nervous system, but also that on the circulation and respiration.

The second part gives a useful account of its adulteration with leaves from other plants. Rough figures of transverse sections illustrate how by presence or absence of certain tissue elements and arrangements we can distinguish the true from the false.

Part III. is chemical; it tells us what tea contains, and how the green and black varieties differ. The latter are usually much richer in the characteristic alkaloid caffein or thein, which M. Biétrie decides to consider as identical; several of the methods of estimating the alkaloid are described. The last part contains short diagnoses, so to speak, of a large number of "teas," with hints for distinguishing them by physical characters.

We quite enjoyed reading the book, though it has the French failing of falling to pieces as soon as opened.

MESSRS. BAILLIÈRE & SONS, of Paris, have just issued a new catalogue of zoological works, referring especially to fishes (and fisheries), reptiles, and batrachians; also a useful catalogue of phanerogamic botany.

WE gladly welcome the second volume of Mr. Blake's *Annals of British Geology*, containing abstracts of the books and papers published during 1891. The editor's comments, which were not always gratefully received, are now relegated to footnotes. The abstracts are generally well done, but it seems a pity that so much space is occupied by lists of names taken from British Museum catalogues.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Professor Dr. Antonio Borzi, of Messina, has been appointed ordinary Professor of Botany and Director of the Botanic Gardens at Palermo.

Dr. Fausto Mori, hitherto extra-ordinary Professor of Botany in the University of Sassari, succeeds Professor F. Tornabene as extra-ordinary Professor of Botany in the University of Catania.

Professor Friedr. Oltmanns, Assistant at the Rostock Botanical Institute, has been appointed extra-ordinary Professor of Botany in the Philosophical Faculty at Freiburg-im-Breisgau.

The Chair of Vulcanology at Catania, rendered vacant by the death of Professor Silvestri, has been abolished. We are glad to learn, however, that a new Chair has been founded in the University of Naples, to which Dr. H. J. Johnston Lavis has been appointed.

Mr. W. R. Ogilvie Grant has been promoted to the rank of First-Class Assistant in the Zoological Department of the British Museum. He has just completed the manuscript of the volume of the Ornithological Catalogue relating to the Game Birds.

In accordance with an Imperial decree, issued on February 9, the city of Dorpat (or Derpt) is henceforth to be known as Jurjeff. The name of one of the oldest and most renowned universities in Russia is thus changed. It is merely one more step towards the Russianising of the Baltic province.

The *Botanical Gazette* of January last informs us that the State University of Iowa has sent Professor B. Shimek to Nicaragua "to follow the route of the canal as near as practicable and make a general investigation of the country, its general character (fertility, climate, &c.), its people, its geology, its flora (special attention being paid to the cryptogamic flora), and its fauna." As the Professor is expected back in Iowa City with his collections "not later than April 1, 1893," he must indeed be a rapid worker to render a satisfatory account in all these departments.

The new University of Chicago is making a grand start, and we heartily wish it success. The following is a list of the teaching staff, so far as geology is concerned: T. C. Chamberlin, Head Professor of Geology; R. D. Salisbury, Professor of Geographic Geology; J. P. Iddings, Associate Professor of Petrology; R. A. F.

Penrose, Jr., Associate Professor of Economic Geology; C. R. Van Hise, Non-resident Professor of Pre-cambrian Geology; C. D. Walcott, Non-resident Professor of Palæontologic Geology; W. H. Holmes, Non-resident Professor of Archæologic Geology; George Baur, Assistant Professor of Palæontology (Biological Department); Edmund Jussen, Docent in European Stratigraphy. If other departments of knowledge are represented on anything like the same scale, this will be the most completely equipped University in the world. The geological professors propose to start a monthly magazine, which will be issued under the auspices of the University.

WE much wish that space allowed us to print in full the strong circular-letter of protest addressed by Dr. John Young to Lord Kinnear, as chairman of the Scottish Universities Commission, on the "omission of Geology from the subjects for which it is proposed to endow chairs in Glasgow University." At present the teaching is not the duty of any Professor in the University, and it has been carried on by means of lectures, by Dr. Young himself, under the Honyman-Gillespie Endowment. For twenty-seven years Dr. Young has divided the Natural History chair into two subjects—Zoology and Geology—and he says sorrowfully, "I have given the best years of my life to minimise the evils of a duplicate commission, or, rather, professorship, and it is hard indeed to find that my earnest appeal on behalf of my chair, my emphatic evidence that I cannot keep pace with two sciences—either of which is enough for one man's energy—have evoked no hint of help from a Commission appointed to improve the teaching in the Scottish Universities." The University has, strangely enough, divided the History chair into Civil and Ecclesiastical, but ignores the claims of Geology. Dr. Young concludes:—"I fervently hope that before I demit office, Zoology and Geology may be adequately provided for, and my successor spared the weary task which has been mine for so many years."

IN a new storey recently added to Firth College, Sheffield, Professor Denny is provided with accommodation for the work of the Biological department to the extent of a laboratory and museum combined, and a lecture theatre communicating with it. The dissecting benches at present available for work seat about thirty students. It is satisfactory to find the number of students steadily increasing every year. In the development of a teaching museum Professor Denny has so far devoted the very limited fund at his disposal to the acquisition of osteological preparations and embryological models, which already fill the museum cases provided. The rooms are fitted with electric light throughout. Hitherto the biological work of the College has been carried on in the museum of the School of Medicine.

WE notice that the President and both Secretaries of the Geological Society this year are graduates of St. John's College, Cambridge. For a long time this college has been prolific of geological students—indeed, we believe we might say that it has produced more geologists than all the other Cambridge Colleges together. This is probably owing in the main to the influence of Professor Bonney, when tutor of the college. Of the University honours in geology, all the Sedgwick Prizes except one, and all the Harkness Scholarships except two, have been won by Johnians.

ON February 14 the President of the College of Surgeons, Mr. Thomas Bryant, delivered the Hunterian Oration on the occasion of the centenary of the death of John Hunter. Mr. Bryant paid a high tribute to Hunter's efforts to establish surgery as a science, and referred to his wide knowledge of zoology and comparative anatomy. The Prince of Wales and the Duke of York honoured Mr. Bryant by attending.

WE learn from the *Athenæum* that a Natural History department is being arranged in the Imperial Museum at Constantinople.

MR. GREENE SMITH'S collection of North American birds and of Humming Birds has been given by his widow to the Museum of Comparative Zoology, Harvard College.

THE plans of the new building for the Departments of Comparative Anatomy, Palæontology, and Anthropology in the Paris Museum of Natural History have been approved, and it is expected that the extension will be completed by the summer of next year.

WE regret to record the sudden death, on February 14, of Sir Charles Wathen, through whose generosity the transfer of the Bristol Museum and Library to the city is being effected. On the proposition of Sir Charles, the Bristol Town Council unanimously decided, on the date mentioned, to adopt the Museums and Gymnasiums Act, but before the conclusion of the business the city's benefactor died suddenly from heart disease.

THE annual report of the Curator of the Museum of Comparative Zoology at Harvard College for 1891-92 has just reached us. Professor Agassiz complains that "the time which our professors give to elementary teaching is entirely out of proportion to that allowed to them for higher instruction. Thus the facilities for original investigation which might be attained at the museum, and for what it was primarily intended, have been thrown away for many years, owing to the inability of the authorities to appoint men whose duties should lie in this direction." He complains that it is not the province of the museum to provide instructors; that belongs to the University; and he strongly protests against undergraduate instruction, which threatens to overcome the higher purposes of the institution. Professor Agassiz also has some strong remarks on the ignorance of those who, though unconnected with the staff, compile Government reports or "circulars of information." In this country the interference of the Government is confined to expenditure of money, on which matter Treasury clerks have power to overrule the decisions of the heads of the various scientific institutions. The description of the "Blake" and of the "Albatross" specimens goes on apace. The final portion of the report describes the progress of the Newport Marine Laboratory, of which some photographic views of the rooms are given, and it points out the advantages of certain specified extensions of this institution.

ON the 18th of the month, the "Report of Proceedings with the Papers read at the Third Annual General Meeting, held in Manchester, July 5, 6, and 7, 1892," of the Museums Association was published. The volume (142 pp.) bears the misleading date of 1892, and is edited by Messrs. E. Howarth and H. M. Platnauer. The list of associates is small; thus we notice only three members of the staff of the British Museum (Natural History), but of these one is the Director, who, we are glad to learn, will preside this year—the Association meeting in London. The list of articles in the report is as follows:—The Organisation of a Botanical Museum, by F. E. Weiss; The Cultivation of Special Features in Museums, by Rev. H. H. Higgins; The Colouring of Museum Cases, by E. R. Waite; Museum Notes, by J. W. Carr; Dust in Museum Cases, by T. P. Teale; Arrangement of Rock Collections in Museums, by H. M. Platnauer; several articles relating to Art Collections, and, more important to the specialist, a "Catalogue of Types and Figured Specimens in the Manchester Museum," by H. Bolton. This last and exceedingly useful list is an outcome of the suggestions of a Committee formed at the British Association meeting of 1889, and adds another to the lists of types contained in the Bristol, Bath, Brighton, Cambridge, and other centres already published. But why did Mr. Bolton put in *Cyclus scotti*, H. Woodw., and *Myriolepis hibernica*, Traquair, as new species? In the first place, new species ought not to be described in a publication of this kind, and in the second place they have both appeared before this report was published (*i.e.*, distributed to subscribers), though the unwary are not given this information.

The Croonian Lecture for 1893 will be delivered before the Royal Society of London on March 16, by Professor Rudolph Virchow, of Berlin. The subject is "The Position of Pathology among the Biological Sciences."

Natural Science is represented in the programme of the Royal Dublin Society's afternoooon lectures by Professor W. J. Sollas and Dr. V. Ball, the former of whom is to speak on "The New Geology," and the latter on "The Scilly Islands."

The study of ethnology and anthropology is being taken up in Ireland with some enthusiasm. An anthropometric laboratory has been, for several months, established in Trinity College, Dublin; it is under the supervision of Professor D. J. Cunningham and Mr. C. R. Browne. The latter observer joined Professor A. C. Haddon last autumn on an ethnological expedition to the Aran Islands, in Galway Bay, when numerous measurements of the islanders were obtained. Professor Haddon and Mr. Browne have lately contributed their results to the Royal Irish Academy. A course of popular evening lectures on Anthropology is in progress at the Royal College of Science, Dublin, by Professor Haddon, who has also been lecturing on the subject at Belfast.

The Natural History Society of Northumberland, Durham, and Newcastle-on-Tyne has organised a series of popular Saturday evening lectures, delivered in its Museum. Dr. Embleton, Canon Tristram, Professor G. S. Brady, and Professor M. C. Potter are among the lecturers, and the public are taking full advantage of the facilities afforded them.

The hundredth anniversary of the foundation of the Literary and Philosophical Society of Newcastle-on-Tyne was celebrated by a Conversazione on February 7. The President (Lord Armstrong) performed and explained some electrical experiments illustrating his recent researches; and the senior Secretary (Dr. R. Spence Watson) read a brief historical sketch of the Society. Next to the Literary and Philosophical Society of Manchester, which was founded in 1781, that of Newcastle-on-Tyne is the oldest in Britain. At the time of its foundation, there were, even in London, few learned bodies. The Royal Society was a hundred years old, and the Society of Antiquaries and the Society of Arts were at work, but the Linnean Society, founded in 1788, was the only association in England devoted to the investigation of any single branch of natural science. During the century of its existence the Newcastle Society had formed a great library, containing many almost unique works, and it is sad to have to relate that, on the morning after the Conversazione, nearly the whole of this library, except the Reference Department, was destroyed by fire.

At the anniversary meeting of the Geological Society of London, held on Friday, February 17, Mr. Hudleston was re-elected president, and Mr. J. J. H. Teall elected secretary, in the room of Dr. Hicks retiring. Owing to the feeble health of the President, the ordinary official annual dinner was not held this year. In view of our remarks in January on "Scientific Dinners," we are glad to observe that Dr. Henry Woodward, a vice-president of the Society, initiated a less costly entertainment, which, we understand, was quite informal, and was attended by no less than 90 Fellows and their friends. Dr. Woodward occupied the chair, and was supported by Sir A. Geikie, Sir H. H. Howorth, Professor Maskelyne, Dr. Hinde, Professor Rupert Jones, Professor Lapworth, and the secretaries of the Geological Society.

OBITUARY.

FRANCIS ORPEN MORRIS, M.A.

BORN MARCH 25, 1810. DIED FEBRUARY 16, 1893.

BRITISH Naturalists will learn with regret of the death of the Rev. F. O. Morris, which took place at his residence, the Rectory, Nunburnholme, Yorkshire, on February 16. Of those devoting themselves to the spread of an interest in Natural History pursuits among the unlearned, Mr. Morris had long been a conspicuous figure; and his success was gained both by the charm of his personal enthusiasm and by the numerous, well-illustrated popular writings which enabled him to reach a wider circle. His best known and most valuable works are his "Histories" of British Birds, their nests and eggs, and of British Butterflies and Moths, the publication of the first commencing in 1851. The new school of Biology inaugurated by Darwin and Wallace, unfortunately, never found favour with Mr. Morris, and his long series of miscellaneous writings antagonistic to this school began with a small volume on "The Difficulties of Darwinism," published in 1870.

CORRESPONDENCE.

A HYPOTHETICAL EXPLANATION OF THE RESULTS OF INOCULATION.

MR. BULMAN sets forth (NATURAL SCIENCE, vol. ii., pp. 100–109) the arguments for and against two theories—the "Exhaustion Theory" and the "Antidote Theory." Another explanation has been familiar to me for some time and to this he has not referred; I greatly regret that I am unable to say where I got it from, but I rather fancy some part of it was "evolved out of the depths" of my own "inner consciousness."

It is as follows:—

The attenuation of the virus by cultivation under conditions unfavourable to the microbe's very rapid multiplication amounts to an artificial selection, the too-rapidly-multiplying stocks being exterminated by overcrowding and the poisonous effects of their own products. After several cultures, each raised from the survivors of the preceding ones, an "attenuated virus" is obtained, *i.e.*, a pure culture of those stocks whose rate of multiplication is low.

The selection may, however, act in some other way—the net result being that the final culture consists of those stocks which, however they may behave on gelatine or boiled potatoes or in chicken-broth or the blood of living apes or guinea pigs, are

different from those with which we started: *by selection* a new stock has been produced, in some cases it is more virulent, in other cases less so.

The dependency of the protective influence of inoculation upon the rate of multiplication of the pathogenic microbe will be seen when we leave ectasines and anectasines out of account and consider only phagocytosis.

Whatever the nature of the stimulus may be, Cohnheim's demonstration that diapedesis of leucocytes is the dominant phenomenon of the resultant inflammation, together with their action as phagocytes in presence of microbes, may be taken for granted. At the seat of inflammation, then, there is a terrific battle between two armies, the microbes attacking, the phagocytes defending. In both armies there is a great mortality, and the ultimate result of the battle depends upon the rate at which each army is reinforced—*i.e.*, upon the rate of multiplication of microbes on one hand and on the rate of migration of fresh phagocytes to the scene of action on the other. So long as the microbes have the best of it the fever increases. So soon as the phagocytes become sufficiently numerous to cope with the microbes, destroying them more rapidly than they are produced, so soon does the fever begin to abate.

The hypothesis in question is that the stimulus of the poison not only increases the diapedesis of leucocytes (*i.e.*, transportation of the defending army of phagocytes to the scene of action), but also leads to an increased rate of production of leucocytes within the body, so increasing not only the size of the standing army provided for the defence of the body, but also increasing the rate at which new recruits are added to it.

If the microbe army introduced be only an "attenuated virus," then that army will not have time to increase to unmanageable dimensions before the phagocytes appear on the scene; the disease will, therefore, be easily checked, and the net result will be an increase in the strength of the army of phagocytes, whose duty is the protection of the animal against the attacks of armies of microbes.

I am not a pathologist, and only bring this to Mr. Bulman's notice that he may subject it to that same criticism which has apparently destroyed the two theories with which he dealt in his article in the current number of NATURAL SCIENCE.

C. HERBERT HURST.

Owens College, Manchester,
February 13, 1893.

TO CORRESPONDENTS.

All communications for the EDITOR *to be addressed to the* EDITORIAL OFFICES, *now removed to* 5 JOHN STREET, BEDFORD ROW, LONDON, W.C.

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All ADVERTISEMENTS *to be forwarded to the sole agents,* JOHN HADDON & Co., *Bouverie House, Salisbury Square, Fleet Street, London, E.C.*

ERRATA.

P. 86, line 39. For "pashs" read "parks."
P. 87, line 20. For "Oházal" read "Dhúyal."